The role of executive control in task switching

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Abstract

This thesis addressed the question of whether global, 'executive' control processes are involved in switching between discrete cognitive tasks. The involvement of executive working memory processes in the control of switching was examined, using a combination of cognitive and cognitive neuropsychological methods. In all studies, participants switched unpredictably between two simple tasks, and in some cases they also performed concurrent tasks. The focus throughout was on two putative areas of executive control that may influence task switching, goal-directed advance processing, and the suppression of interference between tasks. The first series of experiments explored whether the central executive of working memory is required to prepare for a task switch, but found no evidence that this is the case, whether an endogenous or an exogenous method of task cueing is used.

The possibility was then raised that cognitive control does not just operate when the task switches, and a further study showed that this is, indeed, the case. However, two experiments using different task cueing methods did not reveal any evidence that executive processes in working memory carry out this control. It did, however, appear that the central executive is required for overall task performance, as opposed to task switching, when the method of cueing requires that participants keep track of and update information about which task is to be performed. The final study examined task switching and executive function in a group of patients with damage to the frontal lobes, and to posterior areas of the brain. Although a number of participants showed evidence of executive deficits, they had no difficulty in switching in a speeded response task. In conclusion, it is argued that local, rather than global, control processes are involved in switching tasks in the present paradigm, and implications are discussed for theories and investigation of executive control.

Author's declaration

I declare that the work in this dissertation was carried out in accordance with the regulations of the University of Bristol. The work is original except where indicated by special reference in the text, and no part of the dissertation has been submitted for any other degree.

Any views expressed in the dissertation are those of the author and in no way represent those of the University of Bristol.

This dissertation has not been presented to any other University for examination, either in the United Kingdom or overseas.

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Chapter 1: Introduction

Task switching and cognitive control

Cognitive psychology is providing an increasingly clear understanding of the processes involved when people perform a wide range of relatively well-defined tasks. However, what happens outside the laboratory is far more complex than this, and in order to understand it better it is necessary to have a theory of cognitive control. This, broadly, refers to the regulation of behaviour by separate mental processes, in order to respond to conflicting environmental demands, and pursue goals. Outside the laboratory, it is unusual to be required to perform single tasks continuously for long periods of time. One essential component of sequences of multiple actions, therefore, is the ability to switch from performing one task to performing another. The experiments reported in this thesis were designed to investigate whether task switching requires the involvement of separate, global 'executive' control processes. The extent to which cognitive control is dissociable from task performance, and whether it is domain-independent, are the broad questions addressed here. A brief review of relevant concepts of executive functioning will first be presented, before describing the specific ways in which they have been investigated experimentally, in the context of task switching.

A selective review of approaches to control processes

Modern concepts of mental control have their roots in earlier work on both attentional selection and the control of movement and behaviour. In a critical review of the area, Allport pointed out two major dichotomies that appeared to have outlived their theoretical usefulness (Allport 1993). The two issues, around which much work had been based, were the questions of early versus late attentional selection, and controlled versus automatic behaviour. These are reviewed very briefly here, and some mention also made of issues in motor control that relate to both. Allport (1993) suggested that an action-based approach to attention and control, as proposed by Norman and Shallice, and by Duncan, can reconcile some of these broader issues (see also Allport 1980; Norman and Shallice 1980; Duncan 1986). This is taken as one starting point in the present thesis, and the former model is described in some detail.

This leads on to a consideration of a second concept of control crucial to the present investigations, that of the working memory model of Baddeley and Hitch (Baddeley and Hitch 1974; Baddeley 1986).

Attention and performance

One part of performing a specified task is attending to the relevant stimuli. For Broadbent, a 'need' for attentional selection was said to arise because mental resources were limited, and the focus was on understanding how selection operates to avoid overloading the subject with information, for example "...selection takes place in order to protect a mechanism of limited capacity" (Broadbent 1958). Subsequently, there have been various different accounts of why there might be attentional limitations, and whether these are unitary (Kahneman 1973; Norman and Bobrow 1975; Posner 1978), or multiple (McLeod 1977; Duncan 1984; Wickens 1984). A related debate has been about whether selection occurs early or late in the processing 'stream' from stimulus identification to response production. This debate has tended to assume a unitary 'locus' of selection, but it has become increasingly clear that selection can occur at different points, depending on the circumstances (Kahneman and Treisman 1984; Lavie 1995). These observations are reflected in more recent approaches to attentional selection (e.g. Desimone and Duncan 1995).

At the other end of this stimulus-to-response processing stream, action planning was generally investigated separately from attentional selection (Lashley 1951; Miller, Galanter et al. 1960). For example, in the literature on movement control, there was an important conceptual opposition between 'open-loop' (feedback-regulated) actions, and 'closed-loop' (pre-programmed) actions (Requin 1980). However it has also been recognised that there is substantial overlap in the control phenomena previously investigated, separately, in terms of selection at the input level (attention), and preparation at the output level (control). This has led to more integrated approaches. It has been suggested, for example, that the notion of the programming of actions be extended to "processes underlying sensory control of motor activity" (Requin 1980). Studies of the effects of cueing and of partial advance information have shown that control can operate at more than one level and on more than one aspect of processing, such as spatial sources of input (Posner 1978), stimulus categories (Posner and Snyder 1975), object characteristics such as colour (Corbetta 1998), and response-modes, responses or response parameters (Rosenbaum 1980).

Control and automaticity

A second important area of theoretical development, in which the trend has also been towards allowing for *different types of control,* has been the exploration of people's ability to perform two tasks at once. No unitary approach to cognitive control is able to explain the finding that there is not always interference between two tasks performed concurrently (e.g. Allport, Antonis et al. 1972). This, as well as investigations of motor control, led to the idea that some tasks 'require attention' and others do not. The work of Shiffrin & Schneider (1977), among others, defined 'controlled' versus 'automatic' behaviour, and investigated in detail how this distinction could explain performance within a visual search paradigm (Schneider and Shiffrin 1977; Shiffrin and Schneider 1977; Treisman and Schmidt 1982). However, this division rests to some extent on an introspective distinction between willed and reflex, or automatic, action, which goes back at least to William James (James 1890).

Although informative about the acquisition of automaticity, and task variables that might play an important role in cognitive control, such work therefore still took 'attention' as a given, causal mechanism, and was more informative about the nature of automatic task performance than it was about control processing. A similar point has been made about work on selective attention (Johnston and Dark 1986; Allport 1993). From introspection, and in numerous experimental contexts, mental capacity appears limited, but this does not mean that cognitive control is best understood as the operation of a *given* processing limitation or limitations. To make this assumption is to assume that apparent task demands and underlying processing are similar in nature (Rabbitt 1999). Such an assumption has shown to be unwarranted by cognitive models that can simulate apparently 'high level' effects using simple underlying architecture (e.g. Plaut and Shallice 1993; Meyer, Kieras et al. 1995). The fact that it is also unhelpful in explanatory terms is illustrated by the statement of Broadbent, that "A shift of the selective process from one class of events to another takes a time which is not negligible compared with the minimum time spent on any one class" (Broadbent 1958). The observation was accurate, but there was no scope within his, or many later models, for understanding how such a shift could take place.

An understanding of behaviour that includes the ability to switch from one task to another requires an information-processing model of *how* cognitive control operates, not just an acknowledgement *that* it operates. The problem was recently expressed as a "theoretical challenge... to join at some point the 'uphill' side of perception with the

'downhill' side of motor control, without requiring the services of a homunculus in-between" (Niebur and Koch 1998). A 'processing homunculus' refers to a model whose explanatory power is reducible to the idea of a little person sitting in the head, who is the fundamental 'self' perceiving the world and making the decisions (Attneave 1960). The realisation that control may be of more than one type, and occur at more than one level, has led to the development of models that are able to begin to answer these questions, by bringing together selection and action control. In this way it is possible to begin to constrain the way that control processes can operate, and attempt to make testable predictions.

Attention-to-action

Norman and Shallice's (1980) influential information-processing model of the attentional control of action brought together earlier work on skilled behaviour with the results of studies of action slips in everyday life and in neurologically impaired individuals. It drew on earlier theories which emphasised the importance of selection *for* maintaining a coherent pattern of behaviour (e.g. Posner 1978). This allows for multiple levels of control. The model's architecture was based on hierarchical theories of action (e.g. Miller, Galantner et al. 1960) in which a system of goals and subgoals at different levels are implemented by learned action programs, or *schemas,* based on the sequential operation of combinations of different 'cognitive units'. Thus well practised, automatic actions can be executed without conscious effort. Schemas are selected by a combination of environmental 'triggers' and a system that combines activation with lateral inhibition, termed 'contention scheduling' (McClelland and Rumelhart 1981; Rumelhart and Norman 1982). Most importantly for present purposes, however, the model also contains a supervisory attentional system (SAS), an executive controller that is able to direct action when contention scheduling fails, in novel situations or where there is conflict between activated schemas. It was suggested that the SAS initiates action itself by direct activation of a relevant schema, as well as monitoring acts carried out automatically. Support for the existence of a global control system included evidence from studies of action slips in healthy people, where (for example) well-practiced sequences of behaviour are carried out at an inappropriate time (Reason 1983). Also cited was a wealth of clinical observation suggesting that people with brain damage, particularly to the frontal lobes, may appear to have selective deficits of planned or willed behaviour (Penfield and Evans 1935; Luria 1966; Shallice 1982). Further consideration of the effects of frontal lobe damage on executive control and

task switching is postponed until Chapter 5.

When control by the SAS fails, behaviour may be either rigid or distractible, depending on the schema-triggering conditions (Shallice 1988). In the latter case, the control of behaviour can be captured temporarily by a number of weak cues if there is no single environmental trigger strong enough fully to activate a schema (Lhermitte 1983). Such control roles ought to be demonstrable in a situation where participants are switching between two different tasks in the same session, where it may be difficult for any one schema clearly to 'win'. One would expect that the SAS would be required to activate a new task schema in anticipation of the arrival of the stimulus. Other factors that would be expected to influence the involvement of the SAS in a task switch are the presence of interference between tasks, and the type or strength of external task cueing. These have been cited as situations in which, in general, it is likely to be required (Shallice 1988). If a schema is strongly triggered but inappropriate under current circumstances, the SAS may be required to prevent or interrupt its execution. In healthy participants, the SAS might play a role in controlling inter-task interference from conflicting external cueing of task schemas. In patients with damage to the frontal lobes, a difficulty in switching away from performing a task may be explained in this way, if the current task is still being triggered by external cues (Shallice 1988; see also Owen, Roberts et al. 1993).

Clearly Norman and Shallice's model in no way solves all of the difficulties raised earlier in theorising control processes. As has been commented upon frequently, the SAS is no exception to the rule that any postulated central control system risks becoming a homunculus, and it clearly requires a good deal of further specification in order to avoid this. However, it has at least generated hypotheses about possible types and roles of executive control, and in no way rules out the possibility that more than one dissociable system may exist, operating across domains to a greater or a lesser extent (Shallice 1994). These are empirical questions, but before describing how they may assist in developing an understanding of task switching, some introduction is necessary to the working memory approach to cognitive control.

Executive control of working memory

A term commonly used to describe the willed, or top-down, regulation of behaviour is that of 'internal' or 'endogenous control' (e.g. Posner 1978; Passingham 1998). It broadly means that an action or behaviour is not governed directly by environmental

cues and triggers, but is self-generated. As Jeannerod put it recently, "Organisms are not only reacting to external... events, they also actively initiate these interactions" (Jeannerod 1997). However, processes responsible for such self-generated action must have access to (at least temporary) representations of immediate and less immediate task goals. This is by definition a memory problem, but an account is required of *how* such representations may be used to control behaviour. Early models of attention, such as that of Broadbent (1958), explicitly addressed questions of short term memory. Many more recent models do not, including that of Norman and Shallice (1980). However, links have been made between short term memory and cognitive control in some various theoretical approaches, including models of attention (Duncan and Humphreys 1989; Desimone and Duncan 1995), and a number of models of active short-term or 'working' memory (Miyake and Shah 1999). Duncan and Humphreys (1989), in accounting for findings from visual search studies, suggested that "to serve as a focus for action, a part of the ... (perceptual input)... must be selected for access to VSTM (visual short term memory)". In the latter context, there was no reason to theorise any involvement of other short term memory systems, but in this thesis a broader view is needed. The focus here is on a specific model of working memory, that of Baddeley and Hitch (Baddeley and Hitch 1974; Baddeley 1986). For reasons that will become clear, this has theoretical links both with the approaches to cognitive control described above, and with task switching. Early work on this model focussed on understanding the specific verbal and visual short-term memory (STM) processes. The 'central executive' (CE) was assumed to control and co-ordinate these 'slave systems', but was initially not specified further (Baddeley and Hitch 1974). Specification of the CE was necessary to indicate that access to long-term memory (LTM) did not require STM systems, as it had in Atkinson and Shiffrin's model, where 'control processes' were part of STM (Atkinson and Shiffrin 1968). More recently, attempts have increasingly been made to explore the functions of the CE, and Baddeley (1986) has suggested that Norman and Shallice's SAS be adopted as a provisional description of how the CE might operate.

Putative functions of the CE have been outlined, of coordinating multiple task performance, including switching from one task to another, selective attention, and the access and manipulation of material in LTM (Baddeley 1996; Baddeley and DellaSala 1996). The case for a role of the CE in task switching was supported by two somewhat indirect sources of evidence. The first was that some kind of executive control may be involved in the concurrent performance of two tasks (Shallice, McLeod

et al. 1985; Baddeley, Della Sala et al. 1997). If this was because of a role in task scheduling, it would be reasonable to expect the involvement of similar control processing when switching between two tasks. Another reason for the interest was that the generation of random numbers appeared to require executive control, and this was thought to be because of a requirement for frequent switching between different overlearnt memory retrieval strategies (Robbins, Anderson et al. 1996; Baddeley, Emslie et al. 1998). Further evidence for a role of the CE in task switching has been derived from the recent finding that random number generation and a 'verbal trails' task, that involves task switching itself, interfere particularly strongly with one another when performed concurrently (Baddeley, Emslie et al. 1998). Studies that have begun to address this notion directly will be described later, but first, evidence from investigations of the processes underlying switch costs will be reviewed, with an emphasis on the contributions of executive or putative executive control functions.

The cost of a switch

Task switching is not only of interest because of the theoretical considerations of task control described above. It has long been known that there are associated performance costs, referred to here as 'switch costs'. Their existence suggests that there are time consuming processes involved which are not required when carrying out single tasks. An early demonstration of switch (or shift) costs can be found in the work of Jersild (1927). Using simple pencil and paper tasks, in which participants worked through stimulus lists, it was shown that alternating between two mental operations took longer than performing the same one throughout. For example, performance was slower when alternating between adding and subtracting pairs of numbers in a list, compared to completing a single list of one type of sum. This effect was usually substantial, but reduced by more than half if the signs were given alongside the numbers (Jersild 1927). These findings were later replicated and extended by a number of investigators, including Spector and Biederman (1976), who also compared performance on lists of pure versus alternating stimuli (the 'list-method').

Shaffer replicated the finding of switch costs using a speeded response task. Individual trials were compared with each other depending on whether the task switched or repeated, so the switch cost was a difference measure between discrete trials in mixed blocks rather than between performance on mixed versus pure blocks

(lists) of stimuli, as had been the case in Jersild's experiments (Shaffer 1965). Performance was affected by switching or repetition of an abstract task rule as well as of the imperative stimulus. Biederman found similar patterns of results to Shaffer's, and Biederman noted that the rule repetition effect was more marked than the effect of repeating the individual stimulus (see also Bertelson 1961; Biederman 1972). Duncan showed a similar effect using a spatial choice task (Duncan 1975).

More recently, there has been a revival of interest in task switching, as a result of a growth of experimental work on cognitive control. These investigations have begun to move from describing the phenomenon of switch costs to discovering what processes generate them, on the basis that the extra time taken for a task switch must represent the operation of some kind of cognitive control. Results have suggested that more than one process is involved. The major focus, in terms of putative executive mechanisms, has been on the extent to which people can prepare in advance for a switch to a new task. This has tended to be opposed to the potential contribution of the carry-over of (for example) negative priming-like and proactive interference effects. The findings of studies looking at preparation for a switch will be reviewed, and work then described which has looked at other aspects of the control of switching, particularly the effects on switching of interference between tasks. This leads on to a discussion of Allport et al's (1994) alternative, 'non-executive' account of switch costs in terms of carry-over effects of earlier performance of other tasks. Included are preliminary discussions of several models that investigators have proposed to account for their findings, although evaluation of the usefulness of these models will continue throughout this thesis. It will be clear from the findings already presented that what constitutes a 'task' in this context has not been defined. This is deliberate, for reasons that will become obvious later in this Chapter. Having considered processing accounts of switch costs in some detail, attempts made recently to look at the possible involvement of executive, or cross-domain, control in switching will be reviewed, before setting the scene more specifically for the present investigations.

Preparation and the control of task switching

Allport, Styles and Hsieh set out to discover whether there was any evidence that switch costs reflect the operation of executive control (Allport, Styles et al. 1994). They reasoned that if this were the case, then people should be able to prepare for the new task in advance, rather than await the arrival of the imperative stimulus.

Participants alternated between colour-naming and word reading in a speeded-response Stroop task, and the time from the previous response to the arrival of the next stimulus was varied (Expt. 5). Discrete trial response times were measured, but switch costs were based on differences between trials in alternating and pure task blocks, as in the studies of Jersild and others. The expectation was that at longer response-stimulus intervals (RSI) preparation for the switch would be possible, and switch costs reduced. However, in their experiment, a reliable reduction was not found, although the trends were in the predicted direction. Previously, Shaffer had also failed to show any effect of foreperiod on switch cost in an RT experiment (Shaffer 1965; Shaffer 1966). In his study, only the S-R mappings were varied in response to precues on each trial, and as already pointed out, switch costs reflected differences between trials *within* blocks.

Allport et al's (1994) 'non-executive' account of switch costs will be described in more detail later. Subsequent studies, however, have shown that, at least under certain circumstances, people can prepare for a task switch. In a cueing experiment by Sudevan and Taylor, although not set up primarily to look at switching between the two tasks, there was a reduction in the RT difference between validly and invalidly cued trials with increasing cue to target intervals (Sudevan and Taylor 1987).

A two-process model of switching

Rogers and Monsell addressed the question of preparation for a switch in more detail (Rogers and Monsell 1995). They carried out a series of studies in which switching occurred predictably between two subtasks, digit classification and letter classification, in runs of two or more trials (the 'alternating runs' procedure). Task switch trials were compared with task repeat trials in the same block, to generate the switch cost. Successive stimuli appeared in a 2 x 2 grid in rotation, so the task to be performed on each trial was indicated both by predictability and by position (e.g. "for the top 2 positions, perform the letter task"). With a range of intervals from 150ms - 1200ms, their results showed significant reductions in switch costs with longer RSIs. This reduction (of about a third) did not, however continue for RSIs greater than 600ms, and there was always a reliable *residual switch cost* at the longest RSI. This suggested that when the RSI was long, all the processing that could take place before the stimulus arrived had had time to do so, and so did not contribute to the switch cost. When the RSI was short, on the other hand, this process contributed to the switch cost, as there was not enough time for it to be completed before the stimulus arrived.

The authors' explanation for these findings was that there are two mechanisms involved in switching task set, an 'endogenous' or internally-driven one, and an 'exogenous' or externally-driven one. The endogenous component of switching is characterised as the operation of a central control mechanism which allows for a task set to be configured, to some extent, before the imperative stimulus arrives. This mechanism was explicitly identified by Rogers and Monsell as analogous to, or part of, the supervisory attentional system (SAS) of Norman and Shallice (1980). Exogenous control of task switching, in contrast, was characterised as a process of 'stimulus-cued completion' of the switch, where completion of a switch is triggered externally, by the arrival of the imperative stimulus. This explanation of the residual switch cost depended on the finding that there was no further speed-up in RT after the first task repetition (Rogers and Monsell 1995, Expt 6). Regarding preparation for a switch, the case for involvement of executive control was supported by the observation that preparation for a switch did not occur when the RSI was varied randomly within blocks of trials. The authors suggested that this was because it was under voluntary control, and in mixed interval blocks participants did not prepare because they were discouraged from doing so by the unpredictability of the intervals.

Meiran also explored preparation for a task switch, starting from a criticism of Rogers and Monsell's method (Meiran 1996). He pointed out that they did not show conclusively that the reduction of switch cost with increasing RSI was the result of processes that *preceded* task execution. This was because their task confounded time to prepare for a trial with the time elapsed since the *previous* trial, in which elements of the previous task set still active might gradually dissipate. This was not a purely hypothetical objection, since work by this group has since demonstrated an influence of the time since the last trial on switch costs (Meiran, Chorev et al. In press, see Chapter 3). Meiran (1996) carried out a series of experiments using a manual keypress task in which stimuli occupied spatial positions on a 2 x 2 grid. The task on each trial was indicated by fully informative cues, appearing at a variable interval (the cue-target interval or CTI) before the imperative stimulus. The total inter-trial interval was constant. Participants switched unpredictably between classifying stimuli according to their horizontal or their vertical position. (Meiran 1996, Expts. 2 & 3). Switch cost was reliably reduced by a longer CTI, even though this was manipulated within trial blocks. This suggested that preparation for task switching can indeed occur, independently of any carry-over effects that may be operating. Again, a residual switch cost was found. The author's interpretation was similar to that of

Rogers and Monsell, with preparatory effects being considered to be executive in nature (Meiran 1996). Reliable preparation for a switch was also found in one experiment using non-spatial tasks (Expt. 5).

A 3-component model of task switching

More recent work by Meiran and coworkers has led to the claim that three component processes contribute to switch costs. Their model is based on a distinction between stimulus set and response set, two aspects of mental set which are said to control the representations of stimuli and responses, respectively. 'Stimulus set' refers to the focussing of selective attention on the relevant stimulus dimension, leading to stimulus identification. 'Response set' is thought to be carried over from the *last* task performed, and is said to be coded by representations of the 'outcome' (e.g. the mental label 'UP' or 'LEFT') of each response. Thus the switch cost comprises firstly, a 'preparatory component', indexed by the reduction in task shift cost which results from increasing the foreperiod (cue-target interval in Meiran's paradigm). This reflects the time taken to establish a new stimulus set. Secondly, the 'residual component' is indexed by task shift cost at very long cue target intervals, and reflects the degree of interference resulting from having adopted (*after* the last response) the wrong response set. Thirdly, the 'dissipating component' is indexed by a reduction in task shift cost by increasing time since the last response. This decay reflects deactivation of some aspect of the task set with time, and although the processes involved are not specified in detail, it is not thought to reflect the operation of cognitive control (Meiran 1999; Meiran, Chorev et al. In press).

This model differs from Meiran's earlier (1996) one, and is cautious in its claims about the involvement of executive control, with no mention of the SAS. Although preparation for a switch usually represents the adjusting of the stimulus set, the model allows for the strategic preparation of response sets under certain circumstances, and is therefore at risk of over-specification. However, the possiblility of different levels of preparation would seem to merit future investigation, given the evidence of similar effects in other contexts cited above. In summary, it appears that, for Meiran, there are two potential contributions of cognitive control to task switching. The first is the operation of attention in stimulus set selection, and the second is the voluntary and strategic operation of different kinds of preparation for a new task (Meiran In press; Meiran, Chorev et al.).

Converging evidence of preparation for a switch

In all of these studies, the effect of preparation on switching was measured using switch costs, a difference measure between RT or error rates on task switch trials, versus task repeat trials. This has the advantage of subtracting out factors such as the predictability of stimulus onset, which can affect individual trial performance (Meiran, Chorev et al. In press). However, not all the evidence for the cost of switching, and for the possibility of preparation for a shift to a different task, comes from effects on switch costs. In an earlier experiment by Neely, switch costs were shown in a lexical decision experiment that manipulated the validity of cueing of word semantic categories, and these were clearly affected by expectancy over a time-scale of about 500ms (Neely 1977). More recently, in studies based on the psychological refractory period (PRP) paradigm, de Jong demonstrated that participants prepared in advance for a specific subtask *order* as well as for a particular subtask (Dejong 1995). This was true whether preparation was the result of sequential effects or explicit cueing. In terms of accounts of switch costs, De Jong explains the residual cost in terms of a failure of motivation, leading to incomplete preparation, on a subset of switches (DeJong, Berendsen et al. 1999). On this account, therefore, no separate process is required to account for preparatory and residual switch costs.

Allport, Styles and Hsieh (1994, Expts. 6 & 7) found evidence of switch costs of a different type in studies using rapid serial visual presentation (RSVP), in which the dependent measure is accuracy of report and no speeded responses are required, although they did not examine preparation in this context. More recently, however, it has also been shown that preparation for a switch is possible in this paradigm (Perry 1997).

Switching in a production rule system

A slightly different approach to dissociating control processes in task switching has been that of Rubenstein, Meyer and Evans. They carried out a series of studies testing a production-system model of switching, which is part of the broader *Executive-Process Interactive Control* (EPIC) model of cognitive control (Meyer, Kieras et al. 1995; Rubenstein, Meyer et al. In press). The experiments compared performance between pure and alternating task blocks, but measured individual trial RT. As in the studies of Rogers and Monsell (1995) and Meiran (1996), evidence was found of two independent processes contributing to switch costs. Experiments used a combination of arithmetic

tasks (with different operations) and pattern sorting tasks (with different rules). Certain variables known to affect task-specific processes, such as stimulus identification and response selection, were manipulated. These were not found to affect the alternation cost. Variables that did affect task alternation were rule complexity, and the presence or absence of task cues (such as '+' and '-' signs). The claim was that this occurred for different reasons than the effects of these variables on RT, which were interactive, the effects on switch cost being additive. The former was was thought to be due to effects of both variables on response selection, whilst the latter was explained in terms of effects on two different 'executive' control mechanisms contributing to switch costs. The term 'executive' appeared to be used by the authors to denote cognitive control that was not necessarily global in nature, different from the definition used here. Further evidence for the separability of control processing from task processing was found from near-zero correlations of its durations with task-process durations across individual participants (Rubenstein, Meyer et al. In press).

These findings were consistent with Rubenstein et al's model, which proposes that the two cognitive control processes involved in switching tasks are 'goal shifting' and 'rule activation'. The former describes the insertion of task goals into 'declarative working memory', and is affected by task cueing and foreperiod (here, RSI), and is said to be able to occur either before or after the arrival of the stimulus. It is therefore endogenous is the sense used by Rogers and Monsell (1995). The latter describes the loading or unloading of task production rules in 'procedural working memory', and is influenced by operation complexity and irrelevant character effects. This is said to correspond to the exogenous process of Rogers and Monsell, and may only take place once the stimulus arrives. The working memory stores in the EPIC model are not the same as those in that of Baddeley (1986). They are assumed here to be entirely different forms of temporary representation, unless empirical findings suggest that this is not the case. As well as predicting the results of Rubenstein et al's experiments, this model can explain various other empirical findings. Examples given were the absence of preparation for a switch in Rogers and Monsell's studies where RSI was randomly varied (due to strategic 'optional postponement' of goal shifting), influence of task-irrelevant information, carry-over effects of the previous trial, and Allport et al's (1994) finding that the 'scope' of switching does not affect switch costs, since switch costs are the same so long as each 'task' requires its own production rule (all of which are discussed in more detail later).

There are, however, some ambiguities in explaining Rubenstein et al's findings in terms of their model, due to other factors that could have been involved. Because a list-method was used, effects apparently on switching might not have been on the difference between a task switch and a task repeat during mixed task performance, as assumed in the interpretation. They could also have been on mixed task performance in general, because of factors that affect *both* task repeat and task switch trials (the 'mixing cost', see Los 1996). For example, increased operation complexity could have caused slowing of *all* responses, not just of task switching, by increasing the working memory load in the mixed blocks. A slightly different, but also important point is that the presence of irrelevant information in the alternating task blocks might also have increased the switch cost via a different mechanism than that responsible for the effects that such interference has on individual trial switch costs. This is supported by Rogers and Monsell's (1995) finding that switch costs were smaller in an experiment where *all* trial stimuli had neutral irrelevant characters, than they were on neutral *trials* in an experiment where *some* of the stimuli had irrelevant characters associated with the other task (Expt. 4). However, there does not seem to be any obvious alternative explanation for the (independent) effect of task cueing on alternation cost. Although the presence of explicit task cues in some lists might reduce the load on working memory imposed by task alternation, it seems unlikely that this would be so clearly independent of operation complexity, if these effects were also on working memory. In fact, other evidence also suggests that WM load is unlikely to have caused a confound here, and these findings will be described later. For now, the intention is not to imply that the list-method is unhelpful, but rather to emphasise the possible factors that might affect the results and the importance, therefore, of converging evidence from different experimental paradigms.

Task interference effects on switching

One factor that has been found, fairly consistently, to influence task switching is the potential for interference between tasks where, as in the classic Stroop task, there is more than one possible mapping of the stimuli to responses (Stroop 1935; see MacLeod 1991 for review). The presence of such interference generally increased the cost of task alternation in Allport et al's (1994) series of experiments. It also increased switch cost in Rogers and Monsell's (1995) studies. In their letter- and number-classification tasks, the letter or number was always accompanied by an adjacent, task-irrelevant number, letter, or 'neutral' character (such as '&' or '%'). In this way,

since both tasks shared the same two keypress responses, on each trial there was either response-congruent, -incongruent or no interfering information from the presence of to-be-ignored stimuli for the other task. As has already been alluded to above, the presence of such irrelevant information had an *overall* effect on switch costs, increasing them on all trials, in comparison with an experiment in which all irrelevant characters were neutral (Expts. 3 & 4). More importantly in the present context, there was also a greater switch cost on trials with congruent and incongrent irrelevant attributes than on the neutral *trials*, within a single experiment (Expts. 1 - 3). This meant that the performance of a task switch was slowed by the presence of interference from *the other task set*, not just from information associated with a conflicting response. Although there were also differences between incongruent and congruent trials, this 'task set cueing' effect, implying task-rule level interference, was of particular theoretical interest (see also Monsell 1996).

In Rogers and Monsell's Experiment 3, preparatory interval and irrelevant attributes were manipulated together, and their influences on task switching were not found to interact. This is potentially of some theoretical importance in understanding the implications of task switching for cognitive control, since in the original Norman and Shallice (1980) model, the SAS is involved both in endogenous task control and in the suppression of irrelevant prepotent tendencies to action. In the context of task switching, if the two can be shown to be independent, this is evidence against an executive process that is responsible for both of these types of control. Rogers and Monsell's finding can be questioned on two separate grounds. Firstly, as manipulation of RSI varies the time during which influences from the previous trial can dissipate, as well as the time available for preparation for the next trial, it is difficult to determine to what extent effective preparation for a switch was taking place. Secondly, in order to ensure that 'preparation' effects were reliable, Rogers and Monsell had manipulated the foreperiod *between* blocks of trials. Since, as mentioned above, different processes may underlie the effects of inter-task interference within- and between-blocks of trials, this study's findings might not apply to the former. In Meiran's (1996) study, although preparatory interval was manipulated less ambiguously, and within blocks, there were no neutral stimulus attributes in his spatial classification task, so there were task-associated irrelevant attributes, and therefore interference between tasks, on all trials. It is therefore not possible to comment on the presence of interference at the level of the task set or rule, although in one experiment the difference between congruent and incongruent trials was found to occur at long, but

not at short, cue-target intervals (Meiran 1996, Expt. 2).

Implications for cognitive models

In the various models of task switching, the effects of interference between tasks has not always been clearly accounted for. Although Rogers and Monsell's (1995) findings appeared to suggest no link between task-set cueing and preparatory effects on switch costs, the way in which the presence of irrelevant characters inflated the 'residual' switch cost was not explained in their two-process account of switch costs. The residual cost was said, as described above, to be due to 'stimulus-cued completion' of a switch. One might suppose that ambiguous stimulus information might slow this process. Rubenstein et al, however, suggested that in their production-rule model, the rule activation stage would be prolonged for bivalent stimuli, because non-neutral irrelevant characters might make it harder to disable the production rules of prior tasks (Rubenstein, Meyer et al. In press). They also linked this to inertia-like effects of prior task performance, such as those described by Allport et al (1994).

Meiran and coworkers have perhaps attempted the most detailed explanation of interference effects on switching, in the context of developing the 3-component model of switching (Meiran 1999; Meiran In press). As pointed out earlier, their accounts of 'congruency' effects refer to interference at the level of responses rather than of task sets, and as such will not be examined further here. Of somewhat more interest, however, is the suggestion of the model that, depending on the type of interference between two switched-between tasks, there may be differing effects on the control processes involved. Either the stimulus sets may overlap (implying that the two tasks require focussed attention to different stimulus attributes), or the response sets may overlap (implying that different 'labels' are mapped onto the same physical responses in the two tasks). If there is no overlap in either case, then that aspect of the task set does not need to be 'reconfigured' for a switch. In one experiment, preparation for a switch was examined in a version of Meiran's original (1996) spatial classification task that included univalent stimuli that could *only* be interpreted as 'up', 'down', 'left', or 'right' (Meiran 1999, Expt. 6). However, although the reduction of shift cost with longer CTI was less for these stimuli than for bivalent stimuli, it was not abolished, and the author's explanation that this was because participants opted to prepare the response sets instead seemed ad hoc in this context. Given that in Rogers and Monsell's neutral-stimulus-only experiment, preparation reliably reduced switch cost, the usefulness of the stimulus-set vs. response-set distinction in the context of

preparation remains in question. Rogers and Monsell's (1995) and Rubenstein et al's (in press) suggestions that interference influences the switch cost independently of preparation seem simpler and more amenable to testing, at present. Meiran's account of the 'residual' switch cost is not of direct concern here, but it is noted that its nature remains controversial. Although adjusting response-set is claimed to affect residual cost (Meiran 1999), de Jong's theory of motivation effects is also given credence (see DeJong, Berendsen et al. 1999; Meiran, Chorev et al. In press). One of the questions addressed by the first study reported here will be the independence of preparation and interference effects on task switching in the paradigm chosen. Before introducing this, however, some further non-executive contributions to switching will briefly be considered.

Task set inertia: carry-over effects on switching

Although evidence that preparation for a switch does occur has shown that Allport et al's (1994) account of switch costs is not the whole story, their case for the importance of more automatic factors was not only based on this finding. They asserted that switch costs were the result of an automatic carry-over effect of previous processing, which they termed *task set inertia*, or TSI. The findings of two experiments indicated an influence of the previous trial or trials on the time taken to switch tasks. Firstly, in the RT study described earlier, switch costs were *greater* for transitions from word reading ('easy') to colour naming ('hard'), than vice versa (Allport, Styles et al. 1994, Expt. 5). This was only the case for stimulus lists where the colour-word stimuli were incongruent. It therefore appeared that shifting from a hard task to an easy task, in terms of strooplike task dominance, led to *smaller* costs than switching from an easy to a hard task, the opposite of what would be expected if the time taken to switch reflected a proactive, executive process establishing the *new* task set. In the latter case one would expect switching to the easier task would be faster. Instead, the pattern of findings was explained in terms of carry-over effects of events on the previous trial. In this situation, stronger imposition of the task set on nondominant trials would lead to proactive interference from that task set affecting the next, dominant trial.

The second source of evidence for TSI was that the cost of switching appeared to be greater when the participant had recently performed *other tasks afforded by the stimuli* (Allport, Styles et al. 1994, Expt. 4). For example, a large cost of switching

between word naming and digit naming (dominant or 'easy' mappings) was found only where participants had, in a previous experimental block, switched between colour naming and group-size naming (non-dominant or 'hard' mappings). The same applied to non-dominant mappings followed by dominant mappings. This pattern not only contrasted with that found in blocks before the competing tasks had been practised, but also was surprising given the usual finding that where the groups of stimuli used for the two tasks are entirely distinct, switch costs are smaller (Spector and Biederman 1976).

The evidence overall does suggest that an important part of the time cost of task switching relates in some way to how strongly the *previous* task set has had to be imposed, rather than the difficulty or time required to set up the *present* task. However, the generality of asymmetric switch costs according to task dominance is in some doubt (Monsell, Azuma et al. 1997). The account also requires a clearer explanation of what is meant by 'strength' of imposition of a task set. Nevertheless, an influence of inertia-like factors on switching is not necessarily in conflict with the finding of preparation for a task switch, given that there is more than one component to the switch cost. In this context, it is worth noting that Allport et al's (1994) carry-over effects appeared to depend on the presence of strooplike competition between tasks (either current or recently-performed).

Executive control and switching

However many processes contribute to a switch cost, and whatever they do, it is also important to know whether the putative executive components interact with any other cognitive operations. In the absence of such evidence, the involvement of executive control processes cannot, by the definition used here, be supported. The importance of determining the independence, or otherwise, of preparation for, and interference effects on, task switching has already been noted.

Task 'difficulty' and switching

As well as looking for evidence of preparation for a switch as a sign of executive control, Allport and coworkers also explored the effects on switching of some manipulations which ought, on a priori grounds, to affect a general capacity-limited control system (Allport and Styles 1990; Allport, Styles et al. 1994). A number of experiments were performed using alternating and non-alternating stimulus lists.

These used two pairs of tasks, each afforded by one set of stimuli: One was Stroop colour words, with tasks i/ to name the colour, and ii/ to read the word; the second was numeric Stroop stimuli, with tasks i/ to name the group size and ii/ to name the value of the digit. A number of pieces of evidence were then used to argue against the involvement of a unitary central executive in task switching. An experiment was conducted to examine the simple prediction that if a unitary control mechanism with limited capacity was involved in task switching, then switching performance would be adversely affected by increasing the difficulty of the component tasks in other ways. It was shown that varying the difficulty of numeric judgments involved in the tasks did not affect the speed of switching, although it did make the tasks harder in the sense of prolonging RT (Allport, Styles et al. 1994, Expt. 2).

A different manipulation of 'difficulty' in the same series of experiments involved varying the number of task dimensions switched between. Numeric stroop stimuli were again used, and the time taken to complete pure versus alternating lists compared. Switch cost was not increased by having to switch between 2 cognitive operations *and* 2 stimulus dimensions as compared with just *one* of these (Allport, Styles et al. 1994, Expt. 1). However, a further, unpublished experiment by the same group, using a very similar task, produced an apparently discrepant finding, and a greater 'scope' of switching *did* slow responding list (Allport and Styles 1990, Expt. 3). Here, participants were required to alternate between a global and a local focus of attention, as well as attending to different spatial locations. The design of the two studies does not permit any comment on what component or components of switching might have been influenced.

In a further experiment, switch cost was compared on alternating word/ number lists in which *both* tasks were either the dominant ones (word reading and digit value naming) or the non-dominant ones (colour naming or digit group size naming) for that set of stimuli. All stimuli were incongruent. The time cost, for whole lists, was found to be the same for both conditions (Allport, Styles et al. 1994, Expt. 3). This seems surprising in terms of the commonly encountered argument that, using Norman and Shallice's (1980) terminology, the supervisory attentional system is required for overcoming strong habitual tendencies in situations in which they are inappropriate to the task. If this were the case, then, if the SAS was required for task switching, the extra load on the SAS from controlling interference should have caused additional slowing switching in the non-dominant number/ word alternation condition. Allport et

al therefore cite this finding as evidence against a unitary central control mechanism. It is, however, explainable in terms of TSI, if the potentially competing 'alternative' task rules in each case did not affect switching because there was no competition between the two tasks within the blocks of trials, and no recent activation of the competing tasks. This fits with the findings of Allport et al's (1994) Experiment 4, already referred to above.

Working memory and task switching

Two groups of investigators have already conducted experiments aimed at exploring possible roles of working memory systems in the control of task switching. Allport and Styles carried out two studies using the strooplike shifting tasks described earlier. Their aim was to test the idea that working memory limitations were responsible for shift costs, through either the need to retrieve task rules or to keep track of task information. Performance was compared on alternating and nonalternating runs of trials, with some participants shifting between 2 and some between all 4 tasks. Stimuli were pairs of global-local number arrays, and participants classified either i/ the largest digit value or group size by side or ii/ the colour of the digits or the frame around the stimuli. There was, perhaps surprisingly, no increase in alternation cost when 4 tasks were used. The authors therefore concluded that shift costs did not reflect WM limitations, but probably resulted from the time required for control shifts to be implemented (Allport and Styles 1990). It is debatable, however, whether keeping track of 4 tasks always performed in the same order would place much of a load on the central executive, since this would probably be within most participants' short term memory span (Baddeley 1986).

A further experiment compared performance on alternating lists of 2 or 4 tasks when combined with a sub-span verbal WM load. More errors were made overall in the load condition, but there was no increase in the alternation cost with load in *either* the 2- or the 4-task condition. This would seem to suggest that either the central executive is not involved in the switching task, or it is not involved at all in the span task, or both. The latter seems unlikely, given that there is evidence of progressive CE involvement in span performance with increasing list-length (Baddeley and Hitch 1974; Baddeley 1986; Baddeley, Emslie et al. 1998). However, as the memory task involved a sub-span load, perhaps the influence was not strong enough to be detected. However, the finding certainly suggests that on-line memory for the stimulus-response mappings in

these tasks does not rely on articulatory verbal coding, unless the participants code 4 tasks in such a way that the load on the phonological loop is no greater than for 2 tasks, which seems unlikely. The absence of an effect of concurrent task performance overall on alternation cost therefore goes some way towards validating Rubenstein et al's (in press) findings, described above. It supports their implicit assumption, that the apparent effect of certain variables on task switching were not secondary to a greater working memory load in the mixed than in the pure task condition.

Switching and the central executive

A recent series of experiments by Baddeley and coworkers have also looked at the role of working memory in task switching using a dual task method (Baddeley, Chincotta et al. Submitted). A comparison was made between completion times for pure and alternating lists of addition or subtraction sums, using pencil and paper. Secondary tasks included random letter generation and verbal trails, both of which are thought to require central executive input (Armitage 1946; Baddeley, Emslie et al. 1998). They had similar disruptive effects on overall, including blocked trial, performance, but did not increase the alternation cost. This was also true of articulatory suppression by repeating the word 'the'. This result supported Allport and Styles' (1990) conclusion that articulatory verbal coding is not required for a task switch, and neither is the central executive. It is important to note that the method used cannot look at factors affecting preparation for a switch. Although taking time to prepare before encoding each stimulus might be possible, the preparation time would add to the alternation cost and more than offset the reduction in individual RTs. A problem that this raises for Meiran's (in press) explanation of switch costs at short CTI is worth a mention at this point. If these simply represent processing that can be carried out either before or after the stimulus arrives, depending on the time available, it is not clear why preparation for a switch continues for up to 500ms, but the reduction in costs is about 150ms at most. This objection can be met by introducing a notion of cue strength, whereby the combination of the task cues and the target act as stronger triggers for the task set than do the task cues alone. More will be said about task cueing in the next chapter, but as far as Meiran's account goes, this can be seen to be something of a post hoc addition.

Theoretical implications for executive control

So far, it can be seen that the study of the control of switching has not demonstrated any conclusive evidence for shared processing with other aspects of cognitive control. The experiments carried out by Allport and coworkers have set out some important ways in which global processes are *not* involved (Allport and Styles 1990; Allport, Styles et al. 1994). Baddeley et al's (submitted) findings imply that, inasmuch as the two secondary tasks required the input of the central executive, the latter did not appear to be involved in the control of switching. It is important to note, however, that the list method cannot separate the ability to prepare for a switch from other processes contributing to switch costs.

The lack of an influence of particular types of task difficulty on shifting performance allows one to dismiss a simple unitary notion of 'difficulty' dependent on a single central limited resource that also controls switching. However, as the earlier discussion of theories of control has revealed, this is not a particular problem. The findings are consistent with the idea that obvious attributes may not be what is important in terms of requiring adjustment, and a what a 'task' is may depend on instructions and context. For example, it might best be regarded as whatever can be covered by just one production rule (Rubenstein, Meyer et al. In press). A role of a cross-domain control mechanism has, however, not been ruled out, given that other manipulations of 'difficulty' produced conflicting results (Allport and Styles 1990; Allport, Styles et al. 1994). The combination of two different perceptual shifts warrants further investigation, but is outside the scope of this thesis. It is also relevant to address the question of executive control in switching directly, by exploring whether processes thought to be involved in such control in other experimental contexts are also involved in task switching. The main point at which control is said to operate during switching, i.e. preparation, has not been investigated independently in this way, and this is what the present studies initially set out to do.

Executive control and preparation for a switch

So far, it has been suggested that executive control is involved in preparation for a task switch, but there is as yet no direct evidence to support this. Rogers and Monsell (1995) based their claim on the a priori argument that preparation ought to require such control, and on the finding that people only prepared for a switch when the foreperiod was kept constant in blocks of trials, which was thought to indicate a

strategic effect. Since this pattern of findings has not been replicated in other studies of preparation for a switch, this argument must be in some doubt, as the strategic effect (if that is what it was) must at least depend on some other factor, possibly cueing method or strength (both of which receive further consideration later). Allport et al (1994) also argued that preparation would constitute evidence of executive control, although they did not find evidence of this themselves. Meiran's (1999) claim that preparation for a switch involves a shift of selective attention seems reasonable, but has not yet received significant support. The only other factor that has been shown to affect preparation for a switch at all selectively is practice. In Meiran's original (1996) study, the fact that practice reduced switch costs more at short CTI was thought to support the suggestion that executive processing was required to prepare for a switch, as the contribution of such processing would be expected to decline with time as performance of the tasks became automatised. However, even if the preparatory component of the switch cost was required some additional control process, and the residual and/ or interference-related components did not, this does not demonstrate that the control process involved is executive, and acts across domains. A few studies have provided findings of relevance to this question.

'Resources' and task preparation

A study by Proctor and Fisicaro suggested that preparation specifically for a task or task rule 'takes capacity' (Proctor and Fisicaro 1977). Participants classified the colour, size or form of stimuli using manual responses with the right hand, and within a block of trials these tasks were either pure, or mixed and randomly ordered. A left handed keypress response also had to be made to auditory signals, occurring at various intervals before and after the visual stimuli ('probe RT' procedure). Some slowing of the probe RT was found in mixed compared with pure trial blocks, when the tone onset was between about 0ms and 500ms after the visual stimulus. This was interpreted as showing that "time and central processing capacity are required to select between stimulus attributes". Despite various methodological issues that have been raised with the probe RT procedure (e.g. McLeod 1980), these findings are of interest in the current context, since they suggest at least some kind of dual task effect correlated with the need to switch between stimulus classification rules. The time-scale of this was also equivalent to that found for task set preparation (e.g. Rogers and Monsell 1995; Meiran 1996). However, the blocked design meant that this effect could have been due to the need to remember more task rules in the mixed trial blocks. Also,

because inter-trial interval was not varied, it is not possible to tell whether the need to prepare the new task set was specifically responsible for the effect on probe RT. The presence of strooplike interference between tasks in the mixed blocks might have contributed, as could response-level interference due to the use of the two hands for responses in the primary and secondary tasks (see e.g. McLeod 1977).

A dual task study using digit-memory and pursuit tracking concurrent tasks has also found evidence that resource demands in switching relate to processing of signals to switch the focus of attention, rather than switching per se, consistent with a role of executive control in switch-preparation (Laabs and Stager 1976).

Task alternatives and preparation

A recent experiment by Meiran's group attempted to test the idea that, if a supervisory attentional system is involved in preparation for a switch, it should be affected by task uncertainty and/or information load (Meiran Unpublished). Preparation for a task shift was compared when switching between either 2 or 4 nonspatial task alternatives, but these were not found to differ reliably. However, the cues for classification of the shape, fill, line tilt and size of a stimulus, were two laterally placed symbols which not only identified the task on each trial but also indicated the responses; for example, in the shape task the left cue was a square and the right cue a circle. Thus, although task uncertainty would have been different in the 2- and 4-alternative conditions, it would seem that information load and retrieval requirements were minimal in both, since participants had only to look at the cues to know what the task rules were.

On spatial and non-spatial shifts of attention

It is also worth noting at this point that in almost all of Meiran's group's experiments, the tasks used involved classification of stimuli in two different spatial dimensions. They were also cued by pairs of arrows presented peripherally above and below (vertical classification task) or to the left and the right (horizontal task) of the grid in which the stimuli appeared. Although any detailed consideration of processes involved in shifts of spatial attention is beyond the scope of this thesis, it is important to note that these processes may well not be the same as those involved in switching attention between non-spatial tasks. Firstly, attentional selection of different types may involve different processes (Corbetta 1998; Lauwereyns 1998). Secondly, the mechanisms involved in shifts of spatial attention are relatively well understood, in terms of

exogenous versus endogenous control (for review see Rafal and Henik 1994). As the foregoing review has demonstrated, those controlling non-spatial attention switching are as yet less well understood, although similar terminology is sometimes used. On Norman and Shallice's (1980) model, exogenous, schema-driven behaviour in spatial and non-spatial domains would be predicted to be entirely independent. However, processes of endogenous control might not be quite so separate, and a reasonable hypothesis might be that both varieties are carried out by the SAS or a similar executive control process (Posner and DiGirolamo 1998). The experiments in this thesis are concerned only with switching between two tasks where no concurrent shift of spatial focus is required. However, one parallel investigation of spatial attention deserves a mention, because of evidence for a role of working memory.

In an experiment by Jonides in a spatial attention shifting paradigm, a working memory load was found to affect endogenous shifts of attention. The impairment was selective and in proportion to the digit load (Jonides 1980). This suggests that the rules linking the central cues to the required shifts of attention had to be held on-line in working memory for the endogenous mechanism to function. However, in terms of the working memory model it does not necessarily entail central executive involvement. If some of the capacity of the phonological loop was needed to maintain the verbal task instructions, this could explain the effect.

The present experiments

The studies in this thesis take as their starting point the hypothesis that executive processing, of a variety similar to Norman and Shallice's SAS, is involved in task switching. In particular, it is postulated that such control is responsible for the preparation-responsive, 'endogenous' component of the switch cost. A dual task method is used to explore the possibility that task switching, and in particular preparation for a switch, relies on the CE of working memory. This links the properties of the SAS with the requirement for the control of working memory representations, and addresses, in a particular way, the idea that the control of switching involves global cognitive control. With reference to the discussions earlier in this chapter, the studies here make no attempt to provide a global account of executive control. In exploring the CE and its functions, the approach has been explicitly to accept that, at present, the notion of executive processing covers so many possibilities that it does have something of the quality of a homunculus. This need not be a problem, since the

aim of experimentation remains the delineation of specific control processes, and making the homunculus redundant (Attneave 1960; Baddeley 1996). A common approach in the literature, is to approach potentially tractable issues of control separately, and avoid making assumptions about the ultimate determinants of, for example, people's strategies (Logan 1985) and goals (Duncan 1986; Duncan, Emslie et al. 1996). However, since the present studies were designed to address the issue of cross-domain control more directly, it was important to view things in a broader context. It was necessary to use a discrete trials method in order to examine the effects of preparation. Unpredictable switching was preferred so that participants' preparation for a given trial would only begin with the arrival of the task cue (or at least that there would not be systematic departures from this, (see Spector and Biederman 1976). A version of Meiran's (1996) paradigm was therefore adopted, so that cue-target interval could be manipulated whilst keeping the inter-trial interval constant. However, the tasks and cueing methods were designed so that spatial attention shifts were not necessary for task performance.

Brief outline of investigations

Chapter 2 addresses the question of whether the Central Executive is needed to prepare for a task switch. Three experiments are described, using a novel task, in which participants switch between classifying lines and classifying shapes. Different methods of task cueing are examined, one exogenous and one endogenous, to determine the effect of cueing on executive control in switching. Following up some of the findings of these studies, working memory and broad executive control issues are set to one side in Chapter 3, and an experiment conducted to explore performance over several successive task repetitions. Chapter 4 then brings together the issues raised in Chapters 2 and 3, and the role of the central executive in task repetition as well as task switching performance is investigated. In the final experimental chapter, a cognitive neuropsychological study is presented, in which possible impairments of control in patients with frontal lobe damage are examined in the context of of task switching and repetition. In Chapter 6, the broader implications of all the studies are considered.

Chapter 2

The central executive and preparation for a switch

Introduction

In this Chapter, methods from two different paradigms are brought together in order to explore the possibility that there is a common executive mechanism involved in the control of working memory and in task switching. Three experiments attempted, in a specific way, to test the relationship of one operationalisation of 'executive control' to another. This exploration stemmed from two arguments. The first was that preparation for a task switch is an executive function, an a priori assumption made by all models of task switching in one sense or another. The second, that executive control in this sense is the role of the central executive of working memory, was the hypothesis tested here.

As described in Chapter 1, control may be referred to as exogenous, to the extent that task performance is directly driven by external stimuli that are associated with certain tasks. Where task control refers directly to the participant's goals or instructions, it is referred to as internally generated, or endogenous. According to the Norman and Shallice model of the control of action, the supervisory attentional system (SAS) is responsible for the endogenous control of action (Norman and Shallice 1980). In the context of task switching, such a mechanism would allow a person to prepare for the new task. However, if control is entirely stimulus-driven, carrying out a switch must await presentation of the imperative stimulus. In the working memory model, it has been suggested that the central executive (CE) is coordinates processing in different slave systems, including switching between different tasks (Baddeley 1996). There is also evidence that working memory, and possibly the CE, plays a part in endogenous shifts of spatial attention (Jonides 1980). The experiments reported here explored the possibility that the same is true for non-spatial task rule shifts.

Before exploring the role of working memory, the first experiment attempted to replicate the finding that at least two processes are required to account for the time taken by a task switch (switch cost). In general, if people have advance information that a switch will be required, they can to some extent prepare for the new task, and

switch costs are reduced (Rogers and Monsell 1995; Meiran 1996). However, a number of studies have demonstrated that there is always a remaining or residual cost of switching to a new task (Allport, Styles et al. 1994; Rogers and Monsell 1995; Meiran 1996). These are the endogenous and exogenous components of the task switch cost of Rogers and Monsell, and Meiran. The first study also explored whether the effect of interference on the switch cost is independent of the effect of preparation. Two further studies then explored the specific hypothesis that the central executive (CE) is needed to prepare for a task switch. These used different methods of task cueing, which were intended to vary the CE load of preparing for a switch.

Features of the paradigm

The general reasons for the choice of paradigm here have been described in Chapter 1. Its main feature was that participants switched unpredictably between the performance of two subtasks, the task on each trial being cued by explicit signals that precede the imperative stimulus. This method was adapted from that of Meiran (1996). The main dependent measure was the RT switch cost, the difference in response time between task switch and task repeat trials in each condition.

Preparation for a switch was manipulated by varying the time between the instructional cue and the task stimulus, the cue-target interval (CTI). The inter-trial interval was kept constant. This meant that if any time-dependent carry-over effects influence switch costs, these would have equal influences on both long- and short-CTI trials, over the constant ITI, and so should not confound the data (such effects receive some consideration later, in Chapter 3). Although most preparation for a switch appears to be completed within about 500ms, the experiments here followed Meiran (1996) in allowing substantially longer than this, 1500ms. The rationale for this was that the majority of response times on short CTI switch trials were faster than 1500ms, suggesting that all of the processing for a switch should easily be complete within this time interval. The 'room for manoeuvre' was included so that the same should be true later, when secondary tasks were added to the basic design. To examine the effects of interference between tasks, the presence of irrelevant stimulus attributes was manipulated in a similar way to Rogers and Monsell (1995), so that interference from unwanted task-sets rather than unwanted reponses could be assessed. Neither of the two subtasks, nor the method of cueing used here, required spatial attention shifts. The reasons for this have been outlined in Chapter 1.

Experiment 1: Non-spatial task switching

Introduction

The main aim of the first study was to replicate the finding of previous task switching experiments, that there are at least two components to the RT switch cost using a non-spatial task. It also enabled some exploration of the role of executive control in the inhibition of irrelevant information. As explained in Chapter 1, on the Norman and Shallice (1980) model, the SAS is involved both in endogenous task control and in the suppression of irrelevant prepotent tendencies to action. In the context of task switching, the question was whether the endogenous component of the switch cost, and the control of interference, rely on a common mechanism. If they do, one would expect that increased switch costs for stimuli with a to-be-ignored attribute would occur mainly when participants have not had time to prepare for the new task, at the short cue-target interval (CTI). The reasoning behind this was that if the endogenous part of a switch is carried out before the stimulus arrives, its processing time does not affect RT where the CTI is long. Therefore, if the same process were also required to suppress interference, the switch cost at short CTI would be increased where there is a stimulus attribute associated with a response in the other task. If, however, different processes are involved, then interference should increase switch cost regardless of the preparatory interval. As explained in Chapter 1, there is conflicting evidence in the literature on this point.

Predictions

In general, predictions concerned RT effects, and error findings were expected to mirror these. Response times were predicted to be greater on task switch than on task repeat trials, overall replicating earlier findings of switch costs. It was expected that task switch cost would be reduced by participants having a long time to prepare, at the long cue-target interval. Despite this, a remaining or residual cost of task switching at the longer CTI was also predicted. It was of particular interest to determine whether this was present when there was no task-set cueing by irrelevant stimulus information, on neutral trials. Task-set cuing was expected to increase the switch cost at short CTI more than at long CTI, supporting the involvement of a common control process.

Method

Participants

24 undergraduate and graduate students from the University of Bristol, and members of a subject panel, took part in the experiment in exchange for cash or course credits. These were aged between 19 and 31 years.

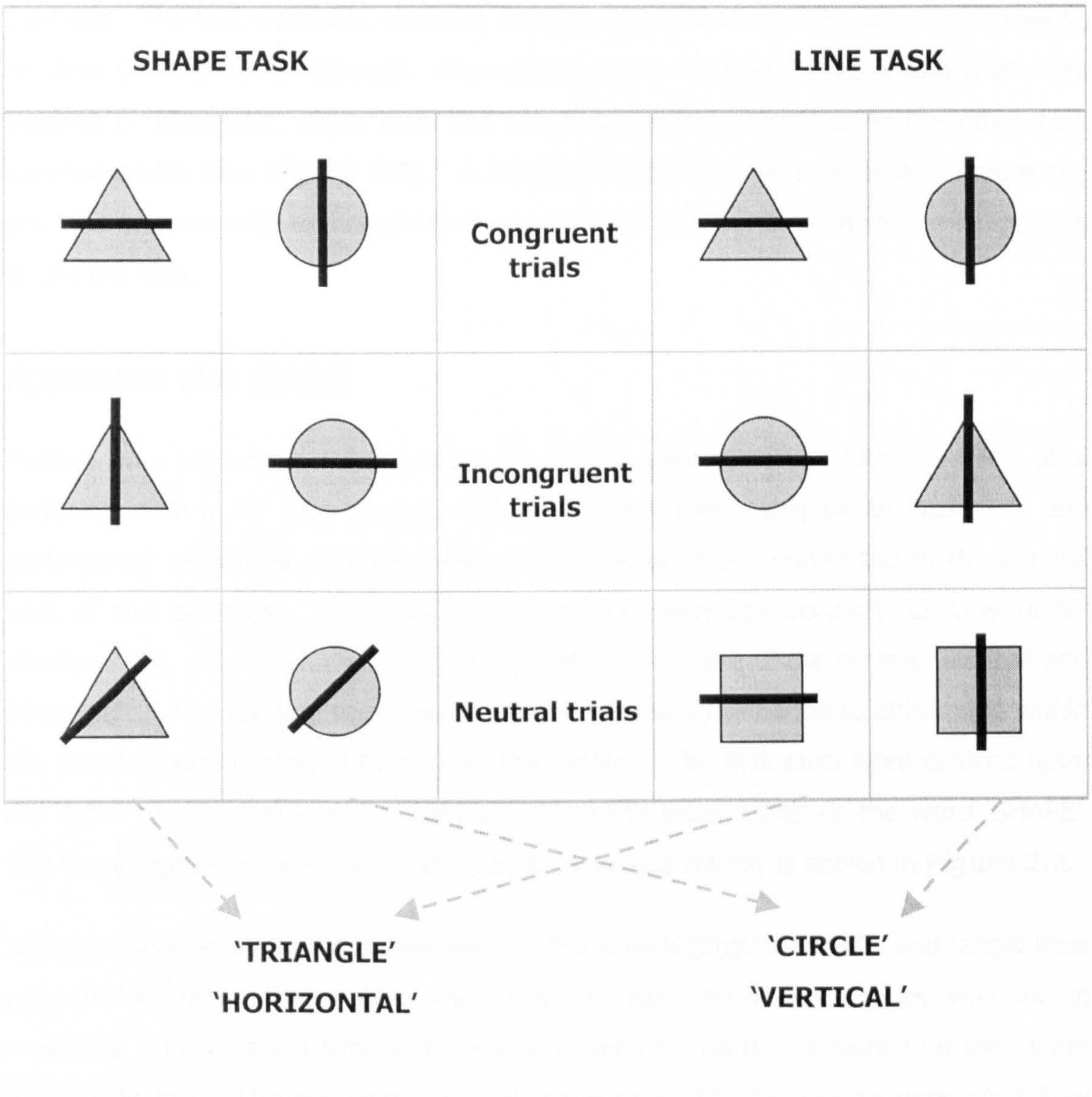

Figure 2.1. Line and shape classification task. The table above shows sample stimuli and responses for half the participants. For the shape and the line task respectively, circles and vertical lines are mapped to a right keypress, and triangle and horizontal lines to a left keypress. Captions indicate the stimulus categorisation for that task.

Task description

Participants were asked to switch between performing two simple speeded response

subtasks. In the shape subtask, participants were required to classify a shape as a circle or a triangle, by pressing the left or the right button on a response box. In the line subtask, the classification was of a line as horizontal or vertical, by pressing one of the same two keys on the response box. On all trials, the stimulus included a to-be-ignored shape or line, depending on the task, so each stimulus appeared as a line superimposed on a shape. Responses were made with the first and middle fingers of the participant's dominant hand, using the same two response buttons for both subtasks. The task-irrelevant attribute could be associated with the same (congruent), or other (incongruent) response. Alternatively, some irrelevant shapes and lines were squares or diagonals, which were not associated with a response in the other task (neutral trials) (see **Figure 2.1**). A fully informative cue was presented before the stimulus on each trial, to indicate to the participant whether to perform the shape task or the line task.

Apparatus and stimuli

Testing was performed with participants seated approximately 40cm in front of a computer with a 15" VGA screen. Software was written using Visual Basic 3.0, and participants' responses were recorded using a response box connected to the parallel port of the computer, a method that gives RT measures accurate to 1ms under Windows 3.1. All stimuli appeared on a panel in the centre of the screen, 6cm tall and 10cm wide. The warning signal was a pair of asterisks, positioned to either side within the panel (approximately 3.75cm from the centre). The task cues were centred upon the same two positions, and were both either the word 'LINE' or the word 'SHAPE'. The target appeared centrally on the panel. A sample display is shown in **Figure 2.2.**

All target shapes were of constant area on the screen (approx. $8cm^2$), and target lines were 4.7cm long, 5mm wide. The colour of both lines and shapes was varied randomly, with different sets of 4 possible colours for each to ensure that they were visually distinct. The cue words and asterisks were displayed in an uppercase Arial bold font, size 18 (height of letters on the screen was approx. 5mm).

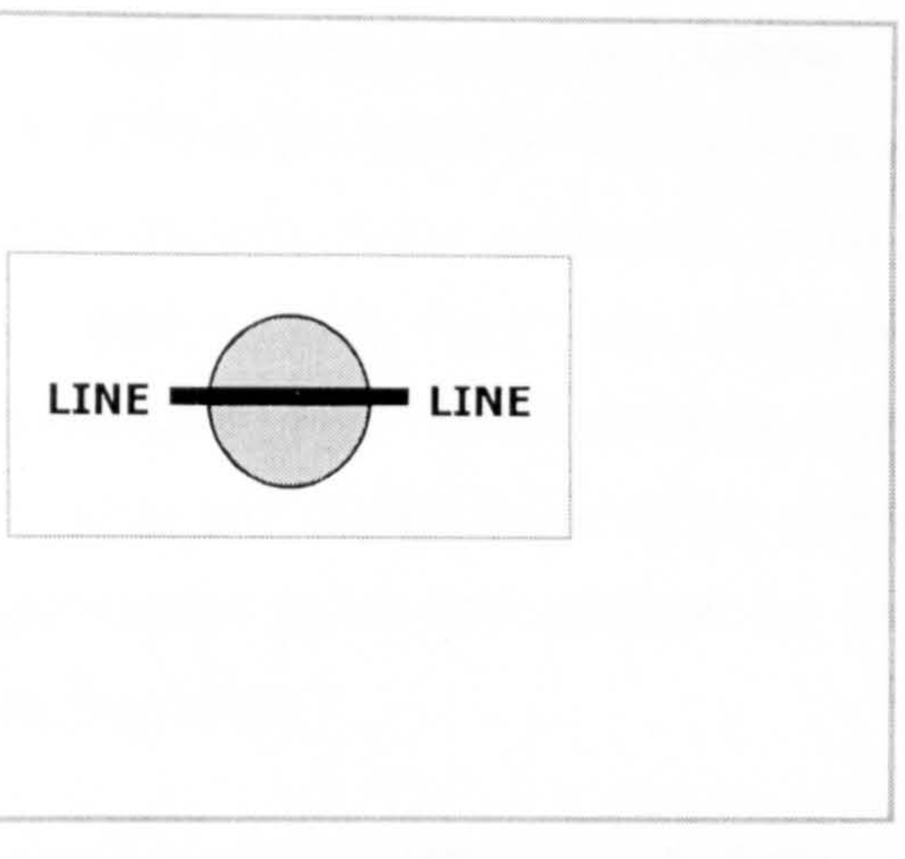

Figure 2.2. A schematic version of the computer display, as it appeared while participants were preparing to respond. The cues and stimulus are shown for a line subtask trial.

Design

The experiment manipulated 3 independent variables in a 2 x 2 x 3 repeated measures design. These were task switching (task switch and task repeat trials), cue-target interval (short and long), and task-irrelevant stimulus attributes (neutral, congruent and incongruent). Task switching was defined as a sequential effect within blocks. Task repeat trials were those where the participant had performed the same task on the previous trial (line → line or shape → shape), and task switch trials were those where he or she had performed the other task on the previous trial (line → shape or shape → line). The cue-target interval (CTI) had two levels, short and long, and was varied within experimental blocks. Irrelevant stimulus attributes could either be neutral trial, response-congruent, or response-incongruent. This was also manipulated within blocks. The stimulus-response mappings, for the line task only, were counterbalanced across participants, to avoid any unpredicted stimulus-response compatibility effects between the mappings for the two tasks. The order of introduction of the two subtasks during practice was also counterbalanced.

Procedure

The interval between trials, the RSI, was a constant 3000ms. The time from a response until the appearance of the next warning signal was either 2200ms or 1000ms, depending on whether the trial was to have a long or a short cue-target interval (CTI). The cue words appeared 500ms after the warning asterisks, and the target then followed the cue by either a short (150ms) or a long (1500ms) CTI. Both cue words and target remained visible until the participant responded. If the

participant made an error, the word 'Incorrect' was immediately displayed on the screen, followed after a pause of 3000ms by repetition of the same trial. This was to allow participants to 'recover' from errors. If the participant failed to respond after 5000ms, the message 'Please respond every time' was displayed, and the trial repeated in the same way. Participants were asked to fixate centrally throughout. Responses were made with the first or the middle finger of the participant's dominant hand. Each practice and experimental block was initiated by the participant, by pressing a third button on the response box.

Four blocks of practice were given, each consisting of 24 trials, all with a CTI of 750ms. One block contained only shape classification trials and another only line classification trials, and all stimuli were neutral. The interval from the warning signal to appearance of the target was constant throughout the practice blocks at 1000ms. The second 2 blocks of practice consisted of mixed trials of both tasks, which were identical to those in the experiment, except that there were 80% incongruent (and 10% congruent/ 10% neutral) trials. The high frequency of incongruent trials was to ensure that, in order to achieve a high rate of accuracy, participants must learn to respond according to the correct task rule on each trial. They were asked to respond as quickly as possible, whilst remaining accurate. It was specified that they should aim for no more than 1 error per block (94% correct). Feedback was given on error rates after each block. Short breaks between blocks, of no more than 5 minutes, were encouraged to avoid fatigue.

The experiment consisted of 8 blocks of 37 trials each, of which the first was a randomly selected trial which was not logged, and on which CTI was always 750ms. Task order was determined on the remainder of trials according to a fixed pseudorandom sequence, in which equal numbers of trials in the two tasks were arranged such that half of them were switch trials, and half were repeat trials. This order was reversed in half the participants. The trial selection algorithm then ensured that equal numbers of trials of the 6 possible stimuli for each task, and the two possible cue-target intervals, were presented during the experiment in random order.

Results

General points about data analysis

The analysis of results for all experiments reported in this thesis followed the same

general approach. For the purposes of analysing response times, all error trials and those immediately following an error were excluded. Median RTs were calculated for each relevant condition, for each participant, and these scores were then analysed further. Error rates were calculated as the number of incorrect responses for every trial logged for that condition, and are given as percentages. If more than one incorrect response was made by a participant consecutively on a single trial, this was scored as a single error. In general, less detail is given for non-significant findings for error rates than for RT, because they were a secondary measure from a theoretical point of view, and also tend to be less sensitive and subject to floor effects. This is particularly true here, as participants were instructed to prioritise accuracy, to ensure that they responded as far as possible according to the correct task rule. In all analyses, an alpha level of 0.05 is used, unless otherwise specified for post hoc comparisons.

Results are reported for the shape and line subtasks combined throughout this chapter, since there was no theoretical reason to consider them separately. Separate analyses were carried out in case there were unexpected findings, but did not yield any important differences, and are therefore not included here. The same is true of the effects of practice during the experiment, which were explored to ensure that there was no evidence of any interaction effects that might lead to different interpretations of other findings, for example by suggesting possible strategy changes over time. Again, these are not reported further.

Preparation and interference effects on task switching

The analysis concerned the effects on task switching performance of preparation for a switch, and of interference due to irrelevant information associated with the other task (task-set cuing). Median RTs and error rates were analysed using a repeated measures ANOVA, with factors task switching (task switch/ task repeat), cue-target interval (short/ long), and task-irrelevant stimulus attribute (neutral/ congruent/ incongruent).

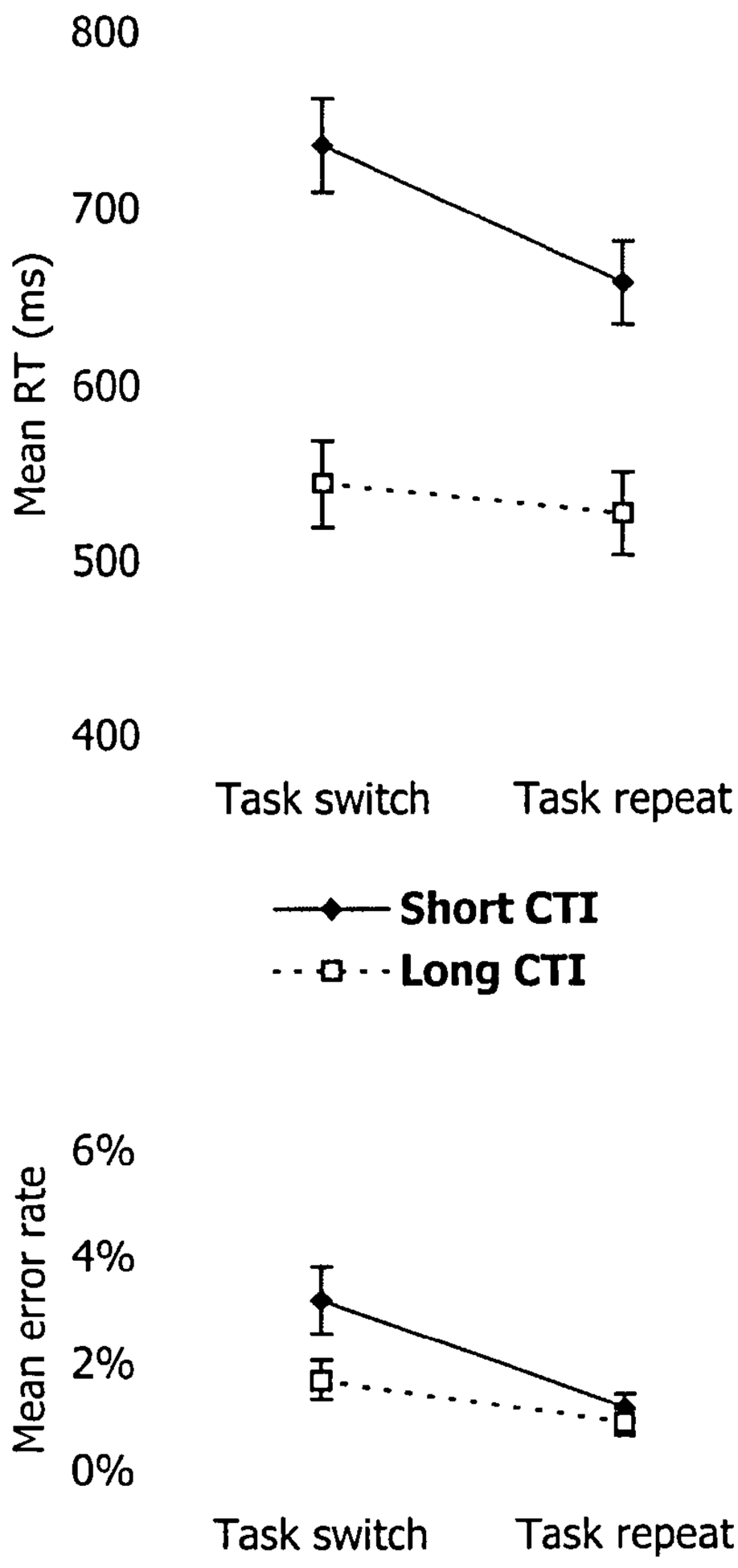

Figure 2.3. The effect of preparation on task switching in Experiment 1. Error bars represent the standard error of the mean.

Effect of preparation

The findings are illustrated in **Figure 2.3.** It can be seen that task switch RT was slower overall than task repeat RT, and that this difference was reduced, but not abolished, with preparation. The analysis revealed reliable main effects of task switching and of CTI, with RT being slower overall on task switch than on task repeat trials ($F(1,23) = 29.89$, $p < 0.001$), and at short than at long CTI ($F(1,23) = 134.35$, $p < 0.001$). As predicted, there was also a significant interaction of task switching with cue-target interval ($F(1,23) = 26.70$, $p < 0.001$). This was because the time cost of switching at short CTI was reliable (for simple main effect, $t(23) = 7.595$, $p < 0.001$), whereas the switch cost at long CTI only approached significance ($t(23) = 1.875$, $p <$

0.08).

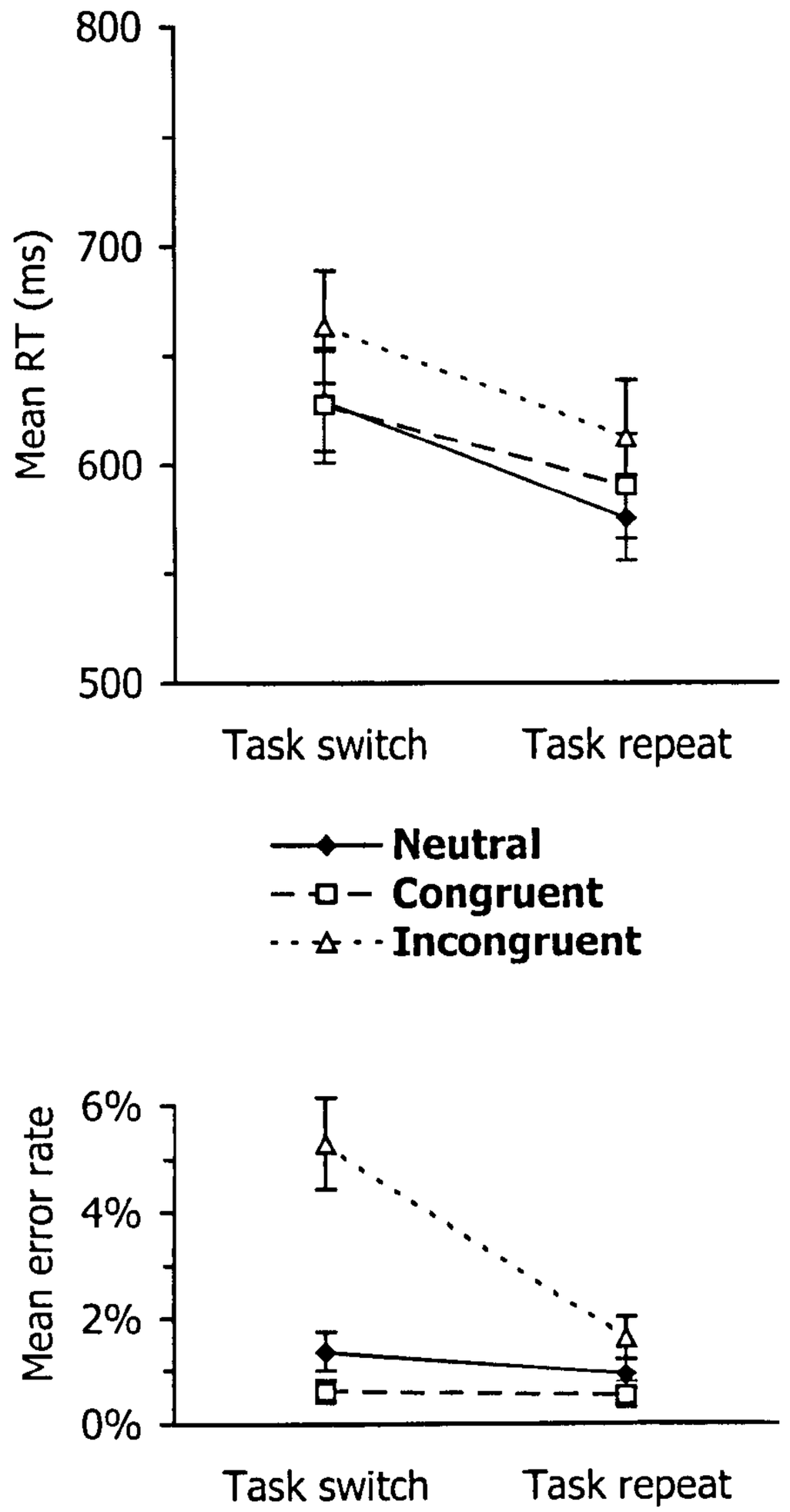

Figure 2.4. The effect of irrelevant stimulus information on task switching in Experiment 1.
Error bars represent the standard error of the mean.

Planned comparisons were made to examine residual RT switch cost on neutral trials only. Although the switch cost at long CTI had not been significant overall, it was reliable for neutral trials alone ($F(1,23) = 9.33$, $p < 0.01$; adjusted $\alpha = 0.017$). The error data also showed a reliable main effect of task switching, with more errors overall on task switch than task repeat trials ($F(1,23) = 11.71$, $p < 0.005$). Neither the main effect of CTI on error rates ($F(1,23) = 4.08$, $0.05 < p < 0.1$) nor the interaction between task switching and CTI ($F(1,23) = 2.85$, n.s.), were reliable, although the trends were in the same direction as for RT.

Effects of interference

The data are shown in **Figure 2.4.** The RT effects did not show the expected greater switch costs on trials with task-set cueing. The main effect of irrelevant information on RT was reliable, but notably absent was the predicted interaction of task switching with this factor for RT (for main effect, $F(2,46) = 134.35$, $p < 0.001$; for interaction, $F(1,23) = 1.41$, n.s.).

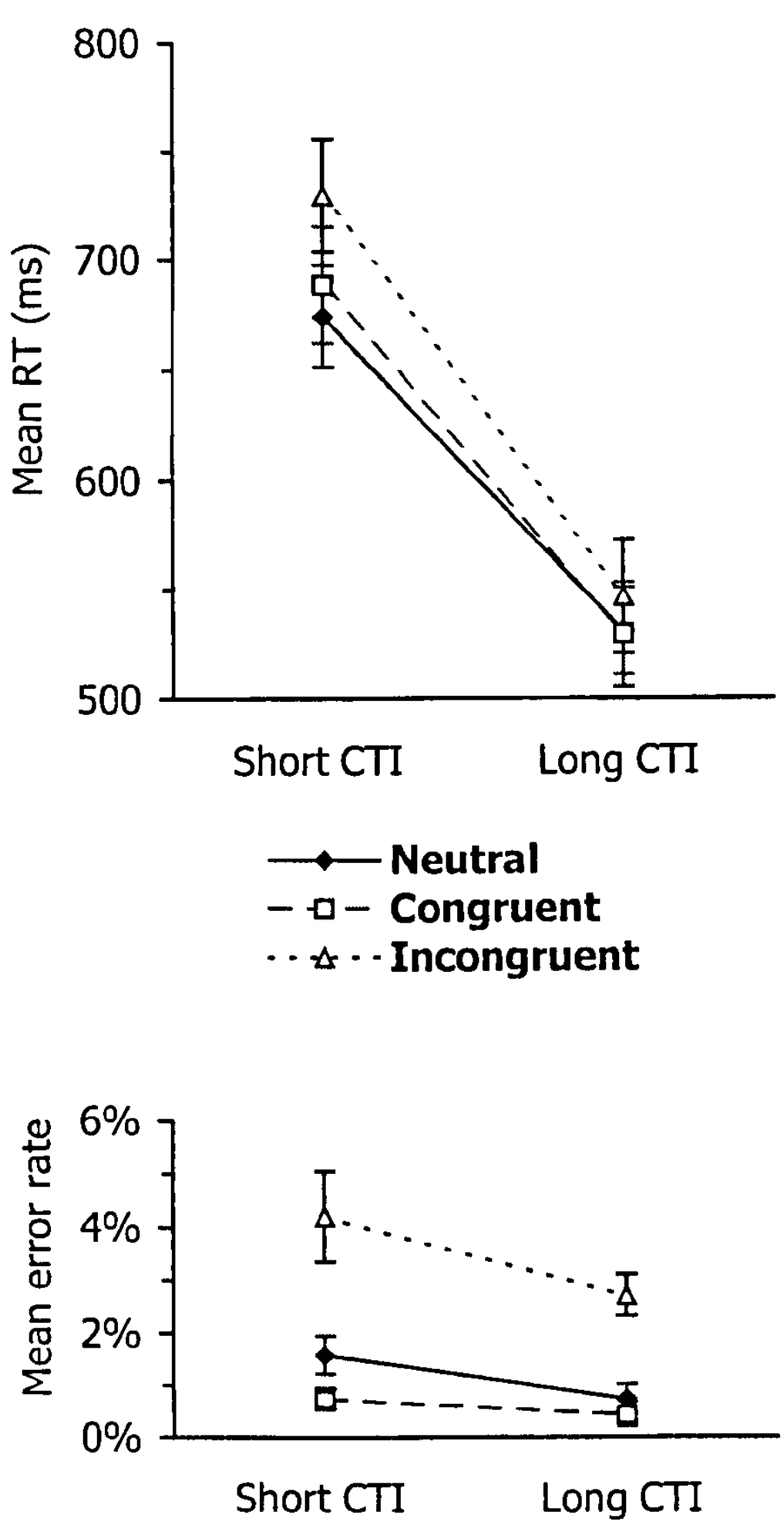

Figure 2.5. The effect of irrelevant stimulus attributes on RT and errors (independent of switching), and the influence of preparation, in Experiment 1. Error bars represent the standard error of the mean.

For errors, however, the main effect of irrelevant information and the interaction with task switching were both reliable ($F(2,46) = 34.72$, $p < 0.001$; $F(2,46) = 14.99$, $p < 0.001$, respectively). Participants responded less accurately on incongruent task switch

trials compared with incongruent task repeat trials (for simple main effect of task switching, $F(1,23) = 14.99$, $p < 0.001$). Where the irrelevant attribute was congruent or neutral, error rates were low and unaffected by task switching (in both cases $F(1,23) < 1$, n.s.).

There was also a reliable interaction of irrelevant stimulus attribute with CTI ($F(2,46) = 5.82$, $p < 0.01$). The data for this relationship, for which no specific predictions had been made, are illustrated in **Figure 2.4**. At long CTI, RT was similar whatever the type of irrelevant attribute, whilst at short CTI incongruent trials were reliably slower than either congruent or neutral trials. The simple main effect of irrelevant attribute was reliable only at short CTI ($F(2,46) = 13.89$, $p < 0.001$; at long CTI, $F(2,46) = 1.28$, n.s.; for pairwise comparison of incongruent vs. congruent trials, $F(1,23) = 15.31$, $p < 0.001$; for incongruent vs. neutral, $F(1,23) = 23.47$, $p < 0.002$; for congruent vs. neutral, $F(1,23) = 1.67$, n.s.).

Summary of findings

This experiment replicated previous findings in two principal ways, showing reliable RT switch costs, with a significant reduction with preparation, as predicted. Although there was no residual switch cost overall, there was some evidence of residual cost on neutral trials alone. This was not expected, and nor was the absence of an interaction between the type of task-irrelevant attribute and task switching for RT. However, the high rate of errors with incongruent irrelevant attributes on task switch trials, compared with task repeat trials, showed some interaction of interference and task switching. Incongruent attributes also caused some RT slowing at short, but not at long, CTI. It is important to note that in general, the direction of the error effects was the same as that of the RT effects. Interference between tasks provided the only exception to this.

Discussion

Switching and preparation

The results of this study supported the broad suitability of the paradigm for investigating processes in task switching. They replicated Meiran's (1996) finding, that when required to switch unpredictably between two non-spatial classification tasks,

participants consistently respond more slowly on task-switch than on task-repeat trials, but that given enough time to prepare for the switch, this 'cost' can be reduced (Expt. 5). This finding of an endogenous component to the switch cost is also in line with those of other studies, using different tasks (Shaffer 1965; Rogers and Monsell 1995; Meiran 1996).

There was some evidence of a residual switch cost, that is a reliable time cost of switching with maximal preparation, although only if neutral trials were considered separately. The presence, to some degree, of a residual switch cost supports findings of previous investigators (Allport, Styles et al. 1994; Rogers and Monsell 1995; Meiran 1996). However, the fact that it was evidently *less* reliable with task-set cueing than on neutral trials was surprising. This did not appear to result from the pattern of findings predicted if a common control mechanism were involved both in the 'executive' part of switching and in controlling interference. If this were the case, there would be an interference-related switch cost that was reduced by preparation, but in fact, task-set cueing did not affect switch cost at short CTI either. Because there was no reliable effect of interference on switch cost at all, it was not possible to address properly the question of the independence of irrelevant character and CTI effects on task switching in this study.

Interference effects

In the present study, RT switch costs were at least as great on neutral as on incongruent trials. This differs from the effect found by Rogers and Monsell (1995), which is a greater switch cost on congruent *and* incongruent than on neutral trials. They accounted for this in terms of the inappropriate triggering of a whole task set rather than just a response, and referred to it as 'task-set cueing'. What was the reason for this discrepancy? It is possible that participants did not follow task instructions about accuracy sufficiently, and so responded according to the wrong task set on a a significant proportion of trials, particularly on task switch trials. Such 'failures to switch' could account for the increased error rate on incongruent switch trials, as on incongruent trials, the two possible responses for the same stimulus are different. On congruent trials, where applying the wrong task rule produces the same response, performance can still be accurate. Average task switch RT (and switch cost) would then be reduced, because of the inclusion of trials where a switch had not, in fact, taken place. However, this is unlikely to account for the difference from Rogers and

Monsell's study, as this showed a similar pattern and overall rate of errors.

In fact, the anomaly here compared with previous studies was more likely to be the neutral trials, because of the task-irrelevant attributes chosen, the square and the diagonal line. These were 'neutral' in Rogers and Monsell's (1995) sense that they were not mapped onto a *response* in the competing task, although they were members of the stimulus categories of shapes and lines. They may therefore have been more strongly associated with the *task set or rule* than Roger's and Monsell's neutral characters in their number and letter classification tasks, which were neither numbers nor letters. If this were the case they would be expected to slow switching in a similar fashion to congruent and incongruent trials, precisely the pattern found here. This interpretation is supported by the fact that irrelevant character effects on RT (as opposed to switching) also did not follow the expected pattern. RT on incongruent trials was slower than on congruent trials, consistent with a slowing due to response competition, but neither differed reliably from neutral trials. This implies that all three trial types probably triggered the competing task to a similar extent. In studies by Monsell and coworkers, such task-set cueing effects have been found on RT as well as on task switching (see Monsell 1996).

Conclusions

Using a novel switching paradigm, this study confirmed that task switching takes time, and that these switch costs are reduced by preparation. There was equivocal evidence of residual switch costs, after preparation. This was consistent with previous claims of the involvement of two or more component processes in generating switch costs (Shaffer 1965; Rogers and Monsell 1995; Meiran 1996). Although the pattern of interference between tasks was not the same as in previous studies, this was probably attributable to the choice of neutral trials. In general, the paradigm appeared to be a suitable one to investigate the role of the central executive of working memory in preparation for a task switch.

Experiment 2: Does central executive load impair preparation for a task switch?

Introduction

This study aimed to explore the role of the central executive of working memory in task switching. Experiment 1 had established a new task in which preparation reliably reduced the time taken for a task switch. This endogenous control of switching has also been demonstrated by other investigators, but the present experiment addressed the question of whether working memory is required for such control to operate, and specifically whether the central executive is involved.

Previous studies of working memory and switching

Studies of working memory (WM) and non-spatial task switching so far have employed Jersild's pure vs. alternating lists paradigm. These experiments have been described in some detail in Chapter 1 (Allport and Styles 1990; Baddeley, Chincotta et al. Submitted; Meiran Unpublished). Effects of WM manipulations on list alternation cost did not suggest any role of verbal WM in switching. The same was true when there was a digit memory concurrent task that probably loaded the CE to some extent (Allport and Styles 1990). However, the list method is unable to distinguish effects on different processes involved, in particular the endogenous and exogenous components of the switch cost. A large increase in the time taken by the former might have affected the alternation cost, but this effect might have been small compared with the effects of load on overall performance in both pure and alternating conditions. A more specific difficulty is that another experiment by this group, using similar tasks but looking at individual response times, did not find a reliable endogenous component to the switch cost (Allport, Styles et al. 1994, Expt. 5). No assumption was made in the present study that different areas of task control which are termed 'endogenous' are necessarily carried out by a single process. Rather, the aim was to establish to what extent the central executive of working memory plays a role in a specific type of 'endogenous' task control, preparation for a switch. Because a discrete trials method was employed, this could be examined selectively.

The same two subtasks were used as in Experiment 1, so the switch was not from one spatial location to another, but from one task rule, and attention to one stimulus

attribute, to another. The operationalisation of interference between tasks was changed, however, because of the unsatisfactory nature of the 'neutral' task-irrelevant attributes used in the previous study. Rogers and Monsell (1995) had included irrelevant information with the stimulus that was visually similar to the attributes used in the competing task, but did not have an associated response. Because Experiment 1 had shown that such attributes may also cause interference between tasks, it was decided to compare trials on which the stimuli were univalent, with those on which the stimuli were bivalent, either response-congruent or response-incongruent. This is consistent with manipulations of task-interference used more recently by Meiran (Meiran 1999).

Dual task method

In order to address this question, a dual task method was adopted. This has been widely used in the working memory literature to dissociate the functions of various components of the model, with consistent results (Baddeley 1986; Logie, Zucco et al. 1990). A number of different tasks have been used to explore the effects of load on the central executive. The task used most frequently has been asking participants to generate random numbers, and this has become something of a benchmark for testing central executive functioning. Other tasks performed in conjunction with random generation (RG) have been found to reduce the randomness of the numbers generated, and this has been used as an index of the central exeutive load of a task. A spatial analogue of verbal RG has also been developed, involving random keypressing (Baddeley, Emslie et al. 1998).

One of the theories as to why RG involves the central executive has been the idea that participants create randomness by switching between different generation strategies (Baddeley 1996). Although recent studies by Baddeley et al have not shown specific evidence for this, at the time the studies here were carried out it seemed desirable to avoid such potentially tautologous definitions of central executive function and task switching. In the latter study, a verbal version of the Trails test (Armitage 1946) was used to load the central executive (Baddeley, Chincotta et al. Submitted). As this was specifically designed to involve task switching, the same argument applies.

Another possible secondary task was low compared with high digit load, because increasing digit load produces increasing disruption of other aspects of CE function, particularly RG (Baddeley and Hitch 1974; Baddeley 1986; Baddeley, Emslie et al.

1998). A digit load was used in a pilot study for the ones described here, because Jonides' (1980) study had employed this method. However, results suggested that the discontinuous nature of the secondary task might be causing participants to adopt particular task combination strategies, such that selective effects on performance at the different cue-target intervals could derive from selective targeting of particular parts of the trial sequence for secondary task performance. Also, there was concern that some were postponing responses, after arrival of the stimulus, in order to carry out the secondary task. The options, therefore, were either to adopt as continuous a secondary task as possible, or to use a paired trial retention interval design for task switch and repeat trials, as used by Jonides (1980). The former seemed preferable, as it enabled minimal changes to be made from the design of Experiment 1.

The tone-counting task used here was designed to fulfil these requirements. Tones were used as external pacing for participants to count upwards either in ones, or in sevens. The rationale was that as counting in ones is highly automatic in most adults, the effects of the former would largely result from articulatory suppression. Counting in sevens (from an arbitrary baseline; see Method) was expected to load the CE, based on evidence that this is involved in mental arithmetic. A study by Logie et al used a dual task method to explore the effect of different working memory loads on a serial addition task with two-digit numbers, that required maintaining a running total (Logie, Gilhooly et al. 1994). This showed disruption of addition by articulatory suppression, and greater disruption with concurrent random number generation. In the latter case, more errors were made, although the magnitude of errors was not increased. In the context of models of mental arithmetic, it was thought that the CE might be involved in performing the calculations required for mental addition, and in producing approximately correct answers (e.g. Dehaene 1992). This was also consistent with a role for the executive in reasoning tasks (Gilhooly, Logie et al. 1993).

Predictions

Components of switch costs

With the alteration of the two subtasks used in Experiment 1 for this study, and different implementation of interference between tasks, it was hoped that the original prediction for Experiment 1, in terms of preparation and interference effects on switch costs, could now be evaluated. A common control process would be reflected in an

interaction of preparation and stimulus valence effects on switch costs. This would mean an increased switch cost at short compared with long CTI where task stimuli were bivalent.

Effects of CE load

In terms of concurrent task effects, the most important comparison in this study was between the low and the high CE load secondary tasks, as this should reflect a CE contribution. The comparison between the single task and low load conditions should mainly reflect the role of the articulatory loop. It was possible that the low load condition would cause measurable disruption of switching performance due to some CE effect of the secondary task, possibly from the act of task combination (Baddeley 1996). In this case, a graded effect from no disruption in the single task condition, through increasing disruption in the secondary task conditions as CE load increased, was expected. Such a graded effect would be unlikely to be caused by interference with the articulatory loop, and in any case the load on this should be roughly similar in the two load conditions (Baddeley 1986).

A close examination of the studies described in Chapter 1 reveals that models of switching have characterised the endogenous component of the switch cost in two distinct ways. Consideration of these led to two possible predicted effects of CE load in this study. One hypothesis assumes that executive control is *always* involved in switching, and the endogenous part of the switch cost is generated by a process that can take place before the target arrives, but if there is not time, it will run to completion after the target arrives (Meiran 1996). On this account, the switch cost at the short CTI is the time taken for the endogenous part of reconfiguration of a task set, plus the time taken by the mechanism that generates the residual switch cost. CE load was therefore predicted to increase the switch cost at short CTI only.

The other claim has been that the executive part of switching is a strategic one, the act of beginning a switch before the stimulus arrives, and not the processing that will otherwise take place after the stimulus arrives. Rogers & Monsell (1995) found that preparation for a switch depended on whether the response-stimulus interval (RSI) was blocked or varied randomly. Switch costs were roughly constant at short RSI whichever method was used, but were only reduced by a long RSI where the intervals were blocked. Rogers and Monsell concluded that preparation for a switch was under strategic control. It should be noted that in Meiran et al's studies (as in Experiment 1),

the reduction in task switch costs was reliable whether the CTI was blocked or random. Attempts to explain this apparent discrepancy are beyond the scope of this investigation. However, a second possible effect of CE load was therefore that participants might become unable strategically to make use of a long CTI to prepare for a switch. If this occurred, switch cost at short CTI would be unaffected, but switch costs at long CTI would be larger (and possibly indistinguishable from short CTI).

This experiment also allowed exploration of a potential inhibitory role of the central executive. Putative executive inhibition functions predicted on the Norman and Shallice (1980) model have already been described. The central executive of working memory has also been claimed to have a role in inhibiting irrelevant information (Engle 1996; Stoltzfus, Hasher et al. 1996). CE load was predicted to cause an increased effect of interference on RT overall. It was also predicted to increase the effect of interference on task switching. Whether such an effect was found only at short CTI, or also at long CTI, was dependent on the independence of the endogenous and the exogenous components of the switch cost (see first prediction).

Method

Participants

24 undergraduate and graduate students from the University of Bristol, and members of a subject panel, took part in the experiment in exchange for cash or course credits. They were aged between 18 and 35 years.

Tasks

Throughout the experiment, participants switched between the same two subtasks used in Experiment 1, shape and line classification. Two secondary tasks were introduced, and performed in different experimental blocks. Both of these involved counting in response to randomly paced auditory tone stimuli. In the 'low load' condition, participants were given a number between 11 and 16, and asked to add one to their running total every time they heard a tone. In the 'high load' condition, participants were again given a number to start with, and were then asked to add seven to their running total every time they heard a tone. They were asked to perform these calculations without speaking aloud, and report the result to the experimenter at the end of the block of trials. In this way, no explicit responses for the secondary

tasks were required during performance of the primary task.

Apparatus and stimuli

The stimuli were the same as those used in Experiment 1, but the implementation of interference between tasks was different (described above; and see **Figure 2.1**, for previous study). In addition, constant pitch auditory tone stimuli for the secondary task conditions were generated by the computer.

Design

The experiment manipulated 4 independent variables in a 2 x 2 x 3 x 3 repeated measures design. These were task switching (task switch and task repeat trials), cue-target interval (short and long), stimulus valence (univalent, bivalent-congruent and bivalent-incongruent), and secondary task condition (single task, low CE load and high CE load). The design was identical to that of Experiment 1, with the addition of the secondary task. This was manipulated between experimental blocks, with each participant performing the three conditions, in the same order, three times consecutively. The order was counterbalanced across participants, using all 6 possible variations.

Procedure

The procedure and instructions for the shape and line subtasks were identical to those employed in Experiment 1. The trial structure and intervals were the same, as was the practice procedure, except for the last of the 4 practice blocks. Participants were asked to perform this block with the 'high load' secondary task, to give them practice at, and confidence in, task combination. The experiment consisted of 9 blocks of 49 trials each. As in the previous experiment, the first trial of each block had a constant 750ms CTI, and was not scored. The tones to be counted in the secondary task conditions were generated by the computer randomly every 3 to 6 seconds. Pilot tests had shown that this timing enabled participants to perform both tasks together satisfactorily. Tone timing was entirely independent of the sequence of events in a trial.

Results

A separate analysis of the single task condition alone is discussed first, to establish the presence of baseline effects before considering the impact of the secondary tasks.

For the analysis of baseline performance, data from the line and shape subtasks were combined under single task conditions only, and an ANOVA was carried out on median RTs and error rates, with the repeated measures factors task switching (task switch/ task repeat), cue-target interval (short/ long), stimulus valence (univalent/ bivalent-congruent/ bivalent-incongruent). For the analysis of the experiment as a whole, the ANOVA included the additional repeated measures factor of secondary task condition (single task/ low CE load/ high CE load).

The data for baseline performance (single task condition) are given in **Table 2.1.** It can be seen that there were reliable RT and error costs of switching, and that these were reduced by preparation. The RT switch cost was also generally greater on bivalent than on univalent stimulus trials, and the error cost on incongruent bivalent trials.

Single task alone

Effect of preparation on switching

Analysis of RT data revealed reliable main effects of task switching ($F(1,23) = 58.14$, $p < 0.001$) and of CTI ($F(1,23) = 70.24$, $p < 0.001$), and a significant interaction of these two factors ($F(1,23) = 6.88$, $p < 0.05$). The simple main effect of task switching was reliable at both cue intervals, but the effect was larger and more marked at short ($F(1,23) = 45.01$, $p < 0.001$) than at long ($F(1,23) = 16.08$, $p < 0.001$) CTI. There was thus a clear residual switch cost overall. Univalent trials were analysed separately, but for these there was no residual cost ($F(1,23) = 3.12$, $p < 0.1$, corrected $\alpha = 0.017$). The error data also showed a reliable main effect of task switching ($F(1,23) = 7.97$, $p < 0.01$), because accuracy was poorer on task switch than on task repeat trials. There were no reliable effects of CTI on errors, including the interaction of CTI with task switching, although there was clearly no error switch cost at long CTI.

Effect of interference on switching

For RT, analysis showed a reliable main effect of stimulus valence ($F(2,46) = 39.36$, p

< 0.001), and a significant interaction of task switching and valence ($F(2,46) = 3.51$, $p < 0.05$). The interaction arose because of a difference between univalent and bivalent trials. For the comparison of univalent with congruent trials, the interaction of task switching and stimulus valence was reliable ($F(1,23) = 5.21$, $p < 0.05$), and the same was true for univalent vs. incongruent trials ($F(1,23) = 16.63$, $p < 0.001$). Congruent and incongruent trials did not differ ($F < 1$, n.s.). Pairwise comparisons showed that overall switch cost was reliable for all three trial types ($F(1,23) = 18.61$ for univalent trials, $F(1,23) = 16.03$ for congruent trials, and $F(1,23) = 42.51$ for incongruent trials, $p < 0.001$ for all). The 3-way interaction of task switching, CTI and valence was not reliable ($F(2,46) < 1$, n.s.).

		Univalent stimuli		**Congruent bivalent stimuli**		**Incongruent bivalent stimuli**	
		RT (ms)	% error	RT (ms)	% error	RT (ms)	% error
Long CTI	**Task switch**	527 (72)	1.1 (0.5)	599 (119)	0.9 (0.6)	620 (119)	4.7 (1.2)
	Task repeat	508 (65)	0.3 (0.3)	562 (114)	2.0 (0.9)	559 (93)	0.8 (0.6)
	Switch cost	18 (10)	0.9 (0.6)	37 (21)	-1.1 (1.0)	62 (19)	3.9 (1.4)
Short CTI	**Task switch**	608 (63)	1.7 (0.8)	789 (170)	1.6 (0.8)	818 (163)	6.8 (2.1)
	Task repeat	564 (45)	0.7 (0.5)	681 (130)	0.2 (0.2)	695 (128)	2.3 (0.9)
	Switch cost	44 (9)	1.0 (1.0)	108 (28)	1.4 (0.9)	123 (20)	4.4 (2.3)

Table 2.1. Baseline task switching performance in Experiment 2, under single task conditions. Mean RTs and error proportions are shown for task switch and repeat trials according to the cue-target interval, and stimulus valence and congruence. Mean switch costs are also given (average switch – repeat difference). Standard errors of the mean are shown in brackets.

Examination of the error data again revealed a reliable main effect of stimulus valence ($F(2,46) = 9.29$, $p < 0.001$), and a reliable interaction of task switching and valence ($F(2,46) = 4.52$, $p < 0.05$). The simple main effect of task switching was reliable on incongruent trials ($F(1,23) = 7.54$, $p < 0.05$), but not on congruent ($F < 1$, n.s.) or on univalent trials ($F(1,23) = 3.34$, $0.05 < p < 0.1$).

As in Experiment 1, there was a reliable interaction for RT of CTI and stimulus valence ($F(2,46) = 10.69$, $p < 0.001$). This was because bivalent stimuli were associated with more RT slowing at short than at long CTI, compared with than univalent stimuli. The interaction of CTI and valence was reliable for univalent versus congruent ($F(1,23) =$

12.42, $p < 0.005$) and for univalent versus incongruent trials ($F(1,23) = 19.84$, $p < 0.001$), but not for congruent versus incongruent bivalent trials ($F(1,23) < 1$, n.s.).

Summary

The predicted effects on task switching of both preparation and interference were clearly demonstrated in this study. RT switch cost was less at long than at short CTI, but was still reliable overall at long CTI, unlike in experiment 1. However, the evidence for a true residual cost was unconvincing, since switch cost at long CTI was not reliable for univalent stimuli. There was a greater RT switch cost for both congruent and incongruent bivalent stimuli in comparison, but the error cost was only increased by incongruent task-irrelevant attributes. This pattern of findings differed from that of Experiment 1, and replicated that of Rogers & Monsell (1995). There was no hint of the predicted influence of preparation on the effect of irrelevant information on task switching. Preparation did, however, reduce the effects of stimulus valence on RT overall. A related effect had been found in Experiment 1, though the pattern, as with all interference effects in that study, had been different.

The experiment as a whole, and effects of CE load

Secondary task scoring

As the computer recorded the number of beeps produced in each block where a secondary task was performed, it was possible to measure the accuracy of each participant over the block as a whole. These data were analysed, comparing performance over the three blocks of each concurrent task, and comparing the two tasks. This was firstly to check that no major strategic shifts occurred over the course of the experiment in the balance between primary and secondary tasks, and secondly to ensure that this balance was not extremely different for the low and the high CE load conditions, in particular that participants were able to perform both secondary tasks with a reasonable degree of accuracy. The dependent measure in the analysis for the low CE load condition was the ratio of the difference in the number of beeps counted and the number recorded to the total number recorded in that block. For the high CE load condition, a similar ratio was based on the difference between the number of 7's added and the number that should have been added in any block. This analysis revealed simply that accuracy was broadly similar in the two tasks, and that it did not vary significantly during the course of the experiment. 5.1% and 6.5% of

beeps were 'missed' overall in the low and high CE load conditions, respectively ($F(1,23) = 1.12$, n.s.). Secondary task analysis will therefore not be carried out for later studies, unless the theoretical need arises.

The effects of preparation and of interference on task switching in the experiment as a whole, and the influence on these of the concurrent tasks, will be considered first. Effects of CE load that were independent of switching will then be reported.

Task switching and CTI

The data for the experiment as a whole for task switching and preparation are given in **Figure 2.6**. A reduction of the RT switch cost at long compared with short CTI can be seen, which is mirrored to some extent by the error data. The main effect of task switching was significant ($F(1,23) = 39.50$, $p < 0.001$), as was the main effect of CTI ($F(1,23) = 52.85$, $p < 0.001$). Despite appearances, the effect of preparation on task switching was not reliable overall (for 2-way interaction, $F(1,23) = 2.28$, n.s.). However, this was not because of any reliable influence of either secondary task on either task switching (for 2-way interaction, $F < 1$, n.s.) or on the effect of preparation on switching (for 3-way interaction, $F < 1$, n.s.). The interaction of load and CTI was also not significant ($F(2,46) = 2.03$, n.s.). The data for the high CE load condition are given separately, in **Table 2.2.** The apparently anomalous pattern of switch costs at short CTI may account for the weakening of the relationship between task switching and preparation in the experiment as a whole. This will be considered further in the Discussion.

For errors, there was a reliable main effect of switching ($F(1,23) = 18.57$, $p < 0.001$) and of CTI ($F(1,23) = 4.43$, $p < 0.05$), but the 2-way interaction of task switching and CTI was not reliable ($F(1,23) = 1.23$, n.s.).

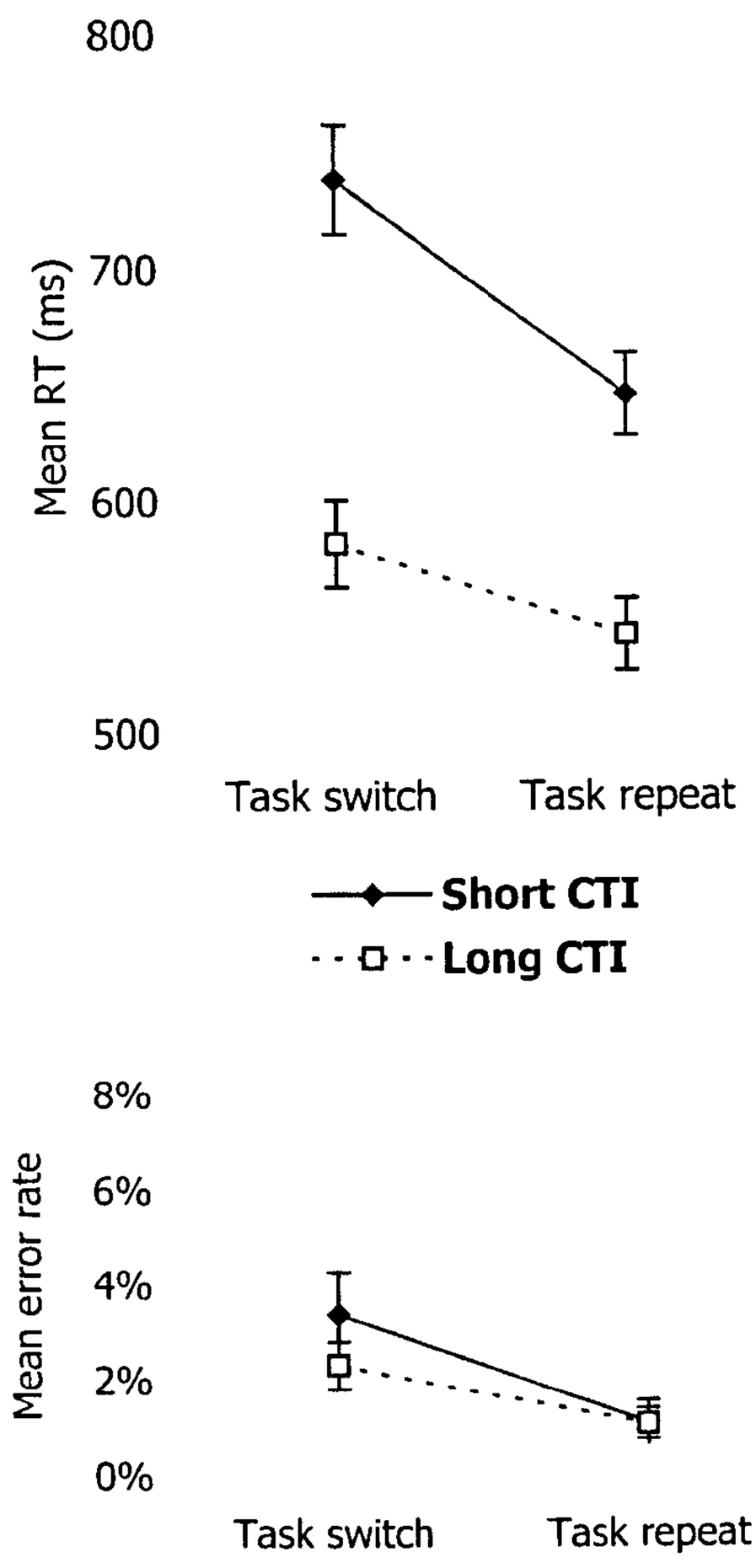

Figure 2.6. Preparation for a switch in Experiment 2. Mean RT and error proportions as a percentage are shown. Error bars represent the standard error of the mean.

Task switching and interference

The effect of stimulus valence on task switching is shown in Figure 2.7. There is some evidence of greater RT switch costs for bivalent than for univalent stimuli. The main effect of stimulus valence on RT was reliable for the experiment as a whole ($F(2,46) = 64.54$, $p < 0.001$), as was the interaction of task switching with valence ($F(2,46) = 3.51$, $p < 0.05$).

The pattern of this interaction was similar to that found for the single task alone, with a reliable simple main effect of switching for congruent ($F(1,23) = 28.27$, $p < 0.001$)

and for incongruent ($F(1,23) = 13.17$, $p < 0.05$) bivalent trials, but not for univalent trials (for $F(1,23) = 3.81$, $0.05 < p < 0.1$). However, the switch cost only differed reliably between univalent and congruent trials. The simple interaction effect of valence and switching here was significant ($F(1,23) = 9.47$, $p = 0.005$), but it was nor for the comparison of univalent and incongruent ($F(1,23) = 1.75$, n.s.) or congruent and incongruent trials ($F(1,23) = 1.37$, n.s.).

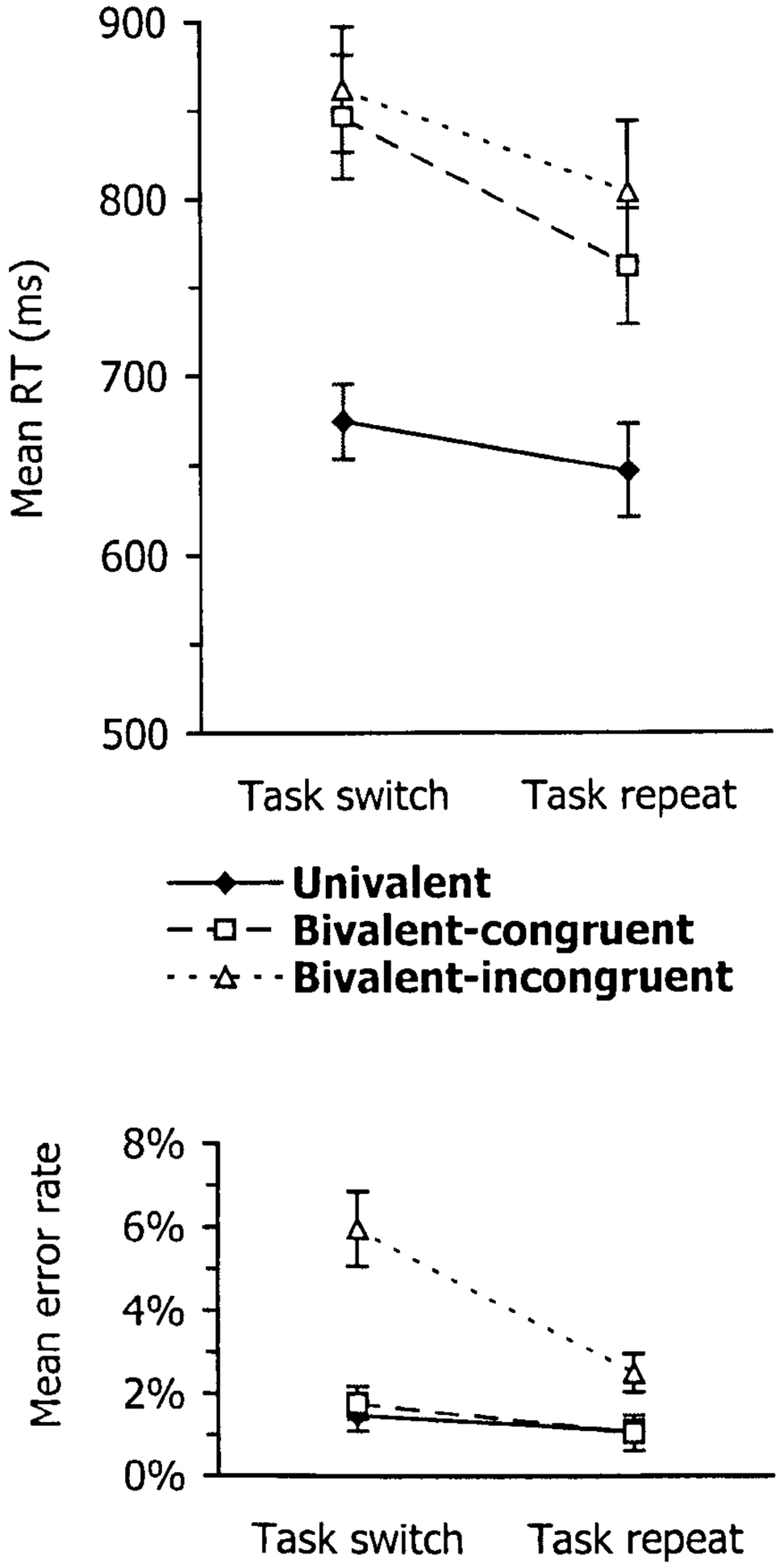

Figure 2.7. Effects of stimulus valence on RT and errors in Experiment 2. Data are collapsed over all three concurrent task conditions. Error bars represent the standard error of the mean.

For errors, in the experiment as a whole, there was again a reliable main effect of stimulus valence, and an interaction of valence with task switching ($F(2,46) = 28.75$, $p < 0.001$, and $F(2,46) = 6.11$, $p < 0.005$). The error switch cost was again reliable only on incongruent trials (for simple main effect of switching, $F(1,23) = 14.14$, $p < 0.001$;

for univalent trials $F < 1$, n.s.; for congruent trials, $F(1,23) = 1.73$, n.s.).

		Univalent stimuli		Congruent bivalent stimuli		Incongruent bivalent stimuli	
		RT (ms)	% error	RT (ms)	% error	RT (ms)	% error
LONG	**Task switch**	803 (45)	1.1 (0.6)	994 (71)	2.5 (1.0)	1072 (83)	6.8 (1.6)
CTI	**Task repeat**	779 (60)	1.4 (0.7)	945 (79)	1.0 (0.7)	1018 (82)	4.3 (1.3)
	Switch cost	23 (47)	-0.3 (0.6)	49 (51)	1.5 (1.1)	55 (65)	2.5 (1.7)
SHORT	**Task switch**	859 (52)	3.0 (1.4)	1142 (84)	2.5 (1.0)	1087 (85)	7.1 (1.5)
CTI	**Task repeat**	844 (60)	1.4 (0.6)	988 (62)	1.0 (0.6)	1106 (88)	3.0 (1.3)
	Switch cost	15 (56)	1.6 (1.6)	155 (57)	1.5 (1.1)	-19 (86)	4.1 (2.1)

Table 2.2. Task switching in the high CE load condition in Experiment 2, showing effects of preparation and of task-irrelevant stimulus attributes. Mean RTs and error proportions are show with standard errors of the mean in brackets. Mean switch costs are also given (average switch – repeat difference).

CE load effect on interference

Although the predicted effects of CE load on switching were not found here, there were other effects on performance. The influence of CE load on stimulus valence effects is illustrated in Figure 2.8, where evidence of increased interference between tasks for RT can be seen in the high CE load condition. Analysis of RT data revealed a reliable main effect of load ($F(2,46) = 43.54$, $p < 0.001$), and a significant interaction of load and stimulus valence ($F(4,92) = 4.48$, $p < 0.005$). The interaction of load with valence and task switching was not reliable ($F < 1$, n.s.), and nor was the 4-way interaction with CTI ($F < 1$, n.s.). The interaction of load with stimulus valence resulted from a difference between the low CE load and the high CE load conditions (for simple interaction effect, $F(2,46) = 3.63$, $p < 0.05$). The single task and the low CE load conditions did not differ in this respect ($F(2,46) = 1.03$, n.s.). In the high load condition, the RT slowing for incongruent bivalent stimuli as opposed to univalent stimuli was greater than in the low load condition (for simple interaction effect, $F(1,23) = 7.47$, $p < 0.05$). There was no load effect for univalent compared with congruent stimuli ($F(1,23) = 2.74$, n.s.), or for congruent compared with incongruent stimuli ($F < 1$, n.s.). There were no reliable effects of the secondary tasks on error rates.

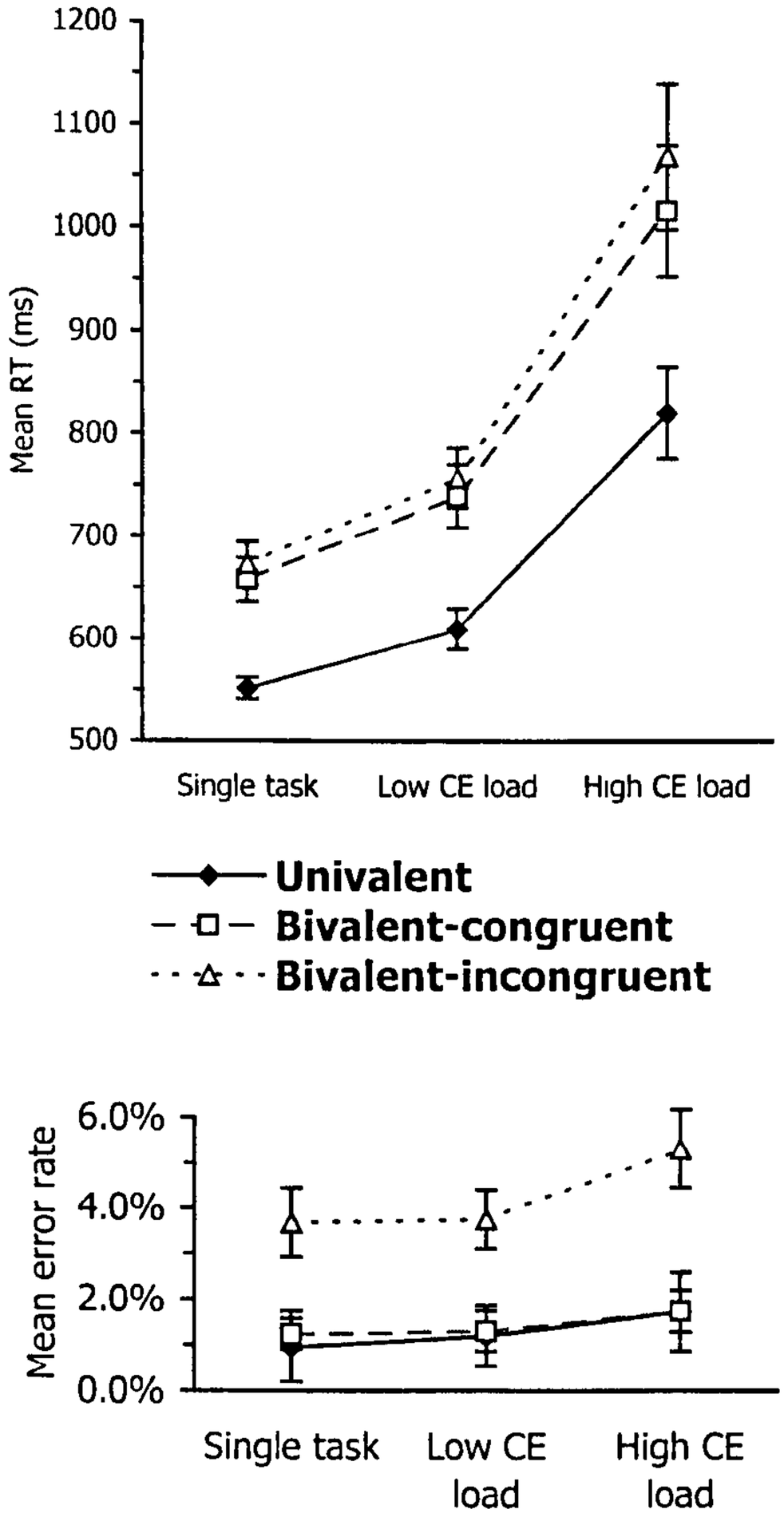

Figure 2.8. Graphs showing the influence of CE load on the effects of interference due to irrelevant stimulus attributes in Experiment 2. Error bars represent the standard error of the mean.

The interaction between CTI and stimulus valence, reliable in the single task condition, was not significant for the experiment as a whole ($F(2,46) = 2.98$, $p < 0.1$). However, there was also no reliable 3-way interaction of CTI, valence and secondary task load ($F < 1$, n.s.).

Summary

The manipulation of interference between tasks in this experiment was successful, as RT switch cost was found to be greater for bivalent than for univalent stimuli, as expected. However the predictions that CE load would interfere with preparation for a task switch were not suppported, despite other load effects on performance. CE load

also did not influence the effect of stimulus valence on switching. The only significant effect of the secondary tasks, other than overall slowing, was to increase the influence of interference between tasks, with bivalent stimuli, on RT (as opposed to task switching) in the high CE load condition. Although this was only reliable for incongruent trials, there was clearly a trend in the same direction for congruent trials, in keeping with the other effects of stimulus valence on RT. Performance was more erratic in the high CE load condition, and the data noisier. Although switch costs were not consistently affected by load, these did appear less marked than in the rest of the experiment, particularly on incongruent trials. Here, switch cost was slightly negative at short CTI, and less than the switch cost at long CTI.

Discussion

'Baseline' task switching and stimulus valence effects

The reduction of task switch costs with preparation that was demonstrated in Experiment 1 was again found in Experiment 2, in keeping with findings in the literature. More importantly, the changes made in the implementation of interference between tasks in this study were successful, in that the pattern of performance demonstrated by Rogers and Monsell (1995) was now replicated. The presence of *any* interfering information from the other task, whether response-congruent or response-incongruent, slowed response times. This effect was greater when the task switched, thus increasing the task switch cost. Monsell and coworkers have described this as *task set cueing*, where interference is occurring at the level of the task set or rule, not at the level of the individual response. Interference at the level of the individual response would yield slower RT on incongruent than on congruent trials, in the manner of the traditional Stroop phenomenon (see Monsell 1996).

Error rates, however, were only affected on incongruent trials. This effect, which again replicates findings of Rogers & Monsell's study, may represent some additional response competition effect. But it is perhaps safer to view it as a conseqence of the fact that task set cueing is difficult to observe in error data, since if a participant responds according to the wrong task set but the responses for the two tasks are congruent on that trial, the response will still be correct. Task set cueing effects on switching were clearly unaffected by preparation. Rogers and Monsell's (1995) findings were similar in this respect, but in their pardigm preparation was manipulated

by varying RSI. This study made clear the independence of the effects of interference and of preparation on task switching when preparation and carry-over effects are not confounded. This was not just a theoretical distinction, since Meiran et al have more recently examined the effects on task switching of varying the interval since the last trial (Meiran, Chorev et al. In press). This issue is considered further in the next chapter.

The findings here did differ from those of Meiran with respect to interference and CTI, however, as his study found only an effect of task-irrelevant stimulus attributes on switch cost at long CTI. However, as stated earlier, this was probably because only congruent and incongruent trials were included, so task set cueing could not be assessed. This experiment clearly showed that, where interference at the level of the task rule is considered, its effect on switching is independent of the preparation for a task switch.

Effect of secondary tasks and CE load

The secondary tasks had been introduced to examine the effect on task switching of disruption of central executive function. No specific effects were found of CE load on task switching, though performance overall was affected, with RT slowing progressively from the single task through the low to the high CE load condition. High CE load also increased the effect of stimulus valence on RT, though not on the switch cost. This broadly fitted the pattern of task set cueing, although the effect was not strong. The only evidence, therefore, of possible executive involvement was in the control of interference. There was no suggestion, however, that this was any different for task switch and task repeat trials, as switch cost was not affected. This study was not designed to assess effects on 'raw' RT, and it is not possible to rule out an effect at the level of stimulus encoding as an explanation for this finding. Although there is no particular theoretical reason to expect such an effect, the bivalent stimuli were more complex visually than the univalent stimuli. Such a consideration would not need to be taken into account were there an effect on task switching, as stimulus processing time is subtracted out when switch costs are calculated.

Most importantly, however, neither of the secondary tasks affected preparation for a task switch, contrary to predictions. The reduction of switch cost did not remain reliable when the experiment as a whole was considered, but inspection of the data revealed no evidence that this was because of a failure to prepare at long CTI. This

was partly because the values for the RT switch costs at both long and short CTI lay between those for long and short CTI in the single task condition, but also because the pattern of task switching performance stood out from the rest of the experiment at the short, not the long, CTI. The overall switch cost at short CTI was lower in the high CE load condition because there was no switch cost for incongruent trials. It is most likely that this was due to the noticable increase in the noise in the data in this condition. However, in the experiments reported in this chapter, a task switch and a task repeat were equally likely on any trial. Could this pattern of performance have resulted from participants being more likely to apply the 'gambler's fallacy' in the high CE load condition, and expect a task switch rather than a repeat? If this were the case, one would have to assume that they began preparing for a switch as soon as the previous trial was over. They would only have time to do this before the next task cue on short CTI trials, where the interval between the previous response and the cue was long. This would tend to reduce switch cost at short CTI, as switch trials would be well prepared, with responses made at a similar speed to those at long CTI, but on repeat trials participants would have to 'recover' from a state of partial preparation for a switch. The only difficulty with this account is that switch cost was preserved for bivalent-congruent trials (though not for univalent trials) in the high CE load condition. This preparation-based effect cannot account for differences in performance according to type of stimulus, i.e. effects of valence, but because the reduction in switch cost occurred selectively at short CTI, it may have influenced the pattern of findings to some extent.

Conclusions

Taking all of this into account, there was no good evidence from this experiment that preparation for a switch was impaired by CE load in either of the ways predicted. Three possible theoretical positions seem consistent with this result. This first is that preparation for a switch is 'executive', in the sense of central endogenous control, but that this is unrelated to central executive function as specified in the working memory model. The second is that preparation for a switch is 'executive' to the extent that the switch is cued endogenously, i.e. not by task cues strongly associated with the task set or rule. If this were the case, the reason for the apparent lack of CE involvement in this study may have been the use of explicit task cues, that trigger the schema or rule strongly enough without requiring much endogenous control. The third possibility is that there is a difference between endogenous and exogenous task cueing in the sense

of the level of demand on 'executive' processing, but that this executive processing is still unrelated to the central executive of working memory. The next experiment aimed to distinguish between these three possibilities, by replicating Experiment 2 as closely as possible, but using a cueing method intended to require greater endogenous control.

Experiment 3: Central executive load and task switching with endogenous task cues

Introduction

This study addressed the question of whether the central executive is only required for task switching when the switching itself demands a high degree of endogenous control. In doing this, the meaning of the notion of 'endogenous' control was to be explored. In Experiment 2, central executive load had no specific effect on switching, and in particular did not impair preparation for a switch. However, this could have been because the task on each trial was indicated by strong external cues that specified the task by name, 'line' or 'shape'. A different task cueing method was used in this study to the previous two, on the basis that switching tasks in response to a task cue that does not specify the task itself, but simply instructs participants to 'switch', engages executive control in a way that the former method does not.

The review of the literature on cognitive control in Chapter 1 has shown that this distinction between exogenous and endogenous task cueing is an important one. This is particularly true of Norman and Shallice's (1980) model, in which the SAS is responsible for control in the absence of strong unambiguous external cues to behaviour. Baddeley's (1986) suggestion, that the central executive of working memory operates in a similar way, can be investigated further in the context of task switching by comparing concurrent task effects when endogenous and exogenous task cues are used.

Previous studies of task cueing and switching

In experiments using the list-method, explicit cueing of the task by the stimulus has been found to reduce the time cost of switching, as measured by the difference in performance between pure and alternating task lists (Jersild 1927; Spector and Biederman 1976). However, there is a methodological problem in some studies, such as Jersild's (1927), as part of the reason for the reduction of alternation cost with exogenous cues is that stimulus preview allows participants to overlap preparation for the next trial with production of the previous response (Spector and Biederman 1976). In addition, this paradigm confounds the strength of task cueing with the potential for

interference between tasks. Where participants alternate between tasks using identical stimulus sets (such as digits), there is no external task cueing, and strong potential for inter-task interference, since all stimuli are clearly bivalent. The use of *simultaneous* task cues (such as digits presented along with '+' or '–') not only strengthens external task cueing, but may also weaken interference, as the signs-plus-digits could now be processed as univalent stimuli. As both of these may be important factors affecting executive control, it is helpful to be able to separate them.

Neuropsychological investigations of patients with Parkinson's Disease (PD) and frontal lobe damage have also suggested that the method of cueing may influence task switching performance. It has been suggested that selective impairments in switching in PD may result from a reduction in general cognitive resources in this patient group, but that these could also be due to additional control processing required for switching when endogenous cues are used (Brown and Marsden 1988; Brown and Marsden 1991; GauntlettGilbert, Roberts et al. 1999). Whatever the reasons for the patients' deficits, these findings support the idea that task cueing is an important variable to explore in trying to define better the control of task switching. The neuropsychological studies will be considered further in Chapter 5.

In Brown and Marsden's earlier (1988) study, data from the healthy controls did not show an increase in task switch cost with endogenous cueing. However, the cue-target interval used was long at 1000ms, so an effect on preparation for a switch would have been missed. In a subsequent experiment with healthy adults, they looked at the effect of working memory load on Stroop task alternation (Brown and Marsden 1991). Random number generation disrupted alternation performance more in the endogenous cueing condition. However, no baseline pure blocks were included for comparison, so it is not possible to know whether this effect was on switching or on cued Stroop task performance. In addition, the finding was not replicated in the control group in their patient experiment (Brown and Marsden 1991, Experiment 2).

Aims and predictions

The cueing method for this experiment was adapted from that used by Brown and Marsden. When comparing 'endogenous' and 'exogenous' task cueing, the present study used external cues which specified that participants should 'switch' or 'stay' on a given trial, rather than naming the task to be performed. Clearly this does provide some external trigger to action, namely, a cue that tells the participant *when* to do

something. This study did not attempt to address questions about the endogenous control of *when* to act, which may have different implications for working memory (Jahanshahi, Jenkins et al. 1995). What it did was to manipulate the extent to which the external cue triggers a particular task set, rule or schema, i.e. tells the participant *what* to do.

Endogenous task cueing and preparation

In order to explore the role of the central executive in preparing for a task switch with endogenous cues, this study also had to establish a baseline for switching performance with this cueing method. No previous study had manipulated preparatory interval where task cues were entirely endogenous, and determined whether preparation can occur in this situation. In Rogers and Monsell's (1995) experiments, task cueing was mostly endogenous, relying on task sequence predictability, although stimulus position acted as an additional cue. Meiran (1996), on the other hand, employed strong external cues like those used here so far, and participants prepared for a switch during a long cue-target interval regardless of trial interval blocking. It is possible that the apparent strategic nature of preparation for a switch in the former, but not in the latter study, was because of this difference in the strength of exogenous task cueing. This is mentioned here because it was possible that in the present study, participants would not prepare for a switch even in the single task condition, because the CTI was randomly varied within blocks of trials. One aim of this study was therefore to check that a reduction in switch costs with preparation can occur effectively with endogenous task cues when the preparatory interval varies on a trial-by-trial basis.

Possible effects of central executive load

The main aim of this experiment, however, was to distinguish the three possible relationships between the central executive, preparation for a switch, and task cueing that were described in the Discussion of Experiment 2. These were tested by looking at the effect of CE load on task switching with endogenous cues.

If the endogenous component of switching is unrelated to the functions of the central executive, then using different task cues would not influence the effects of CE load on switching. In that case, the pattern of findings with respect to load would be just as in the previous study, and CE load would again not affect preparation for a switch. If the CE is required for a task switch only when there are no exogenous task cues, then

preparation for a switch would now be impaired by CE load in one of the ways predicted for Experiment 2. These were either that switch costs at short CTI would be increased because of slower or less efficient preparation for a switch, or that switch costs at long CTI would be as great as those at short CTI, because of a failure strategically to engage the endogenous mechanism before the stimulus arrives.

This study also enabled the effect of endogenous versus exogenous task cueing on preparation for a task switch to be investigated, by an explicit comparison of baseline task switching performance with that in Experiment 2. This comparison would yield interesting information even if CE load did not affect preparation for a task switch. The third possibility was that endogenous task cueing would affect switching in the absence, or independently, of any effect of CE load. This prediction would be evaluated by a quantiative comparison of Experiment 3 with Experiment 2. If the 'executive' control of switching is affected by this variable, but still unrelated to executive function in the sense of working memory, preparation for a switch would take longer with endogenous than with exogenous task cues. Switch cost at short CTI would be greater than in Experiment 2, but CE load would continue to have no effect on this.

Method

Participants

24 undergraduate and graduate students from the University of Bristol, and members of a subject panel, took part in the experiment in exchange for cash or course credits. None had participated in either of the earlier experiments. They were aged between 18 and 39 years.

Tasks

Participants again switched between shape classification and line classification unpredictably. The only difference from the tasks used in Experiment 2 was that the cue words were either 'SWITCH' or 'STAY', to indicate whether participants should switch to the other subtask, or continue performing the same one. On the first trial of each block, however, explicit task cues were given, identical to those used previously, stating whether to begin with the 'SHAPE' or the 'LINE' subtask. If participants made an incorrect response, the error trial was repeated, as before, after a warning message

and a pause, and explicit task cues were also given on such 'error repeat' trials.

Design and Procedure

The design and procedure of this study were identical to those of Experiment 2, with the single exception of cue-type, as described above.

Results

The single task condition was first analysed alone, to determine how the use of endogenous cues affected task switching, before the impact of the secondary tasks was considered. For the analysis of baseline performance, data from the line and shape subtasks were combined under single task conditions only, and an ANOVA was carried out on median RTs and error rates, with the repeated measures factors task switching change format of factors (task switch/ task repeat), cue-target interval (short/ long), stimulus valence (univalent/ bivalent-congruent/ bivalent-incongruent). For the analysis of the experiment as a whole, the ANOVA included the additional repeated measures factor of secondary task condition (single task/ low CE load/ high CE load).

Single task alone

Effects of preparation on task switching

The effect of preparation on task switching for RT and errors is illustrated in **Table 2.3**, along with the effects of stimulus valence. For Experiment 3, as in the previous two studies, there was an overall cost of task switching, and a reduction of RT switch cost with preparation. These findings were reflected in a reliable main effect of switching ($F(1,23) = 33.20$, $p < 0.001$) and interaction of switching with CTI ($F(1,23) = 16.22$, $p < 0.001$). Further analysis is given below, because there were reliable differences between the two studies in this respect (see comparison of performance in Experiments 2 and 3). The main effect of CTI was also reliable for RT ($F(1,23) = 150.67$, $p < 0.001$). The error data showed reliable main effects of switching ($F(1,23) = 12.22$, $p < 0.005$) and of CTI ($F(1,23) = 14.17$, $p < 0.005$). However, the interaction of CTI and task switching was not reliable ($F < 1$, n.s.).

		Univalent stimuli		Congruent bivalent stimuli		Incongruent bivalent stimuli	
		RT(ms)	% error	RT(ms)	% error	RT(ms)	% error
Long CTI	**Task switch**	604 (31)	2.2 (0.8)	625 (26)	1.2 (0.7)	710 (39)	5.6 (1.4)
	Task repeat	542 (26)	0.9 (0.7)	614 (52)	1.4 (0.7)	636 (44)	1.3 (0.8)
	Switch cost	62 (23)	1.3 (0.9)	11 (42)	-0.2 (1.0)	73 (28)	4.2 (1.4)
Short CTI	**Task switch**	937 (69)	2.7 (0.8)	1173 (71)	4.7 (1.6)	1244 (71)	10.9 (2.4)
	Task repeat	758 (38)	1.8 (0.9)	951 (65)	2.0 (0.9)	987 (69)	5.9 (1.8)
	Switch cost	179 (54)	0.9 (1.1)	222 (57)	2.7 (1.8)	257 (39)	5.0 (3.0)

Table 2.3. Baseline task switching performance in Experiment 3, under single task conditions. Mean RTs and error proportions are shown for task switch and repeat trials according to the cue-target interval and stimulus valence and congruence. Standard errors of the mean are shown in brackets.

Response times at long CTI were analysed separately, to determine whether a residual switch cost was present. The simple main effect of task switching was not significant overall ($F(1,23) = 3.56$, $0.05 < p < 0.1$), but it was reliable on univalent trials ($F(1,23) = 7.08$, $p < 0.05$; adjusted $\alpha = 0.025$). The error switch cost at long CTI was not significant for any trial type.

Effects of interference on switching

The effects of stimulus valence on task switching for RT and errors in the single task condition are shown in **Table 2.3**. It can be seen that the switch cost appears to be greater for bivalent trials, mainly at short CTI. The main effect of stimulus valence on RT was reliable ($F(2,46) = 52.23$, $p < 0.001$). However, its effect on switching was not significant (for 2-way interaction of task switching and valence, $F < 1$, n.s.; for 3-way interaction with CTI, $F(2,46) = 1.30$, n.s.). Most errors, as in the previous studies, were on incongruent trials (for main effect of stimulus valence, $F(2,46) = 10.02$, $p < 0.001$). This effect was slightly more marked on task switch trials, although the interaction of task switching and valence was not quite significant ($F(2,46) = 3.08$, $0.05 < p < 0.1$).

Effects independent of task switching

The interaction of CTI and stimulus valence for RT was reliable ($F(2,46) = 26.50$, $p < 0.001$). Details are considered below, when a comparison with Experiment 2 is made, and the relevant data are given then. There was also a reliable interaction of CTI and stimulus valence for errors in Experiment 3 ($F(2,46) = 3.94$, $p < 0.05$). This was because errors on incongruent, and to a lesser extent congruent, bivalent trials were reduced by preparation. The simple main effect of CTI was reliable for incongruent ($F(1,23) = 12.69$, $p < 0.005$) and congruent ($F(1,23) = 4.43$, $p < 0.05$) trials, but not for univalent trials ($F < 1$, n.s.).

The experiment as a whole, and effects of CE load

Effects of preparation on task switching

The data for the effect of preparation on task switching in Experiment 3 as a whole are illustrated in **Figure 2.9**. It can be seen that, across all concurrent task conditions, switch costs were still reliably reduced by preparation. The main effects of task switching ($F(1,23) = 9.94$, $p < 0.005$) and of CTI ($F(1,23) = 144.13$, $p < 0.001$) remained reliable, as did the interaction of switching and CTI ($F(1,23) = 11.53$, $p < 0.005$). This was as predicted. As in Experiments 1 and 2, the simple main effect of task switching was reliable at short CTI ($F(1,23) = 38.72$, $p < 0.001$), but was less marked, in this case not significant, at long CTI ($F < 1$, n.s.).

Preparation for a switch was not, however, influenced by CE load, as neither the the 3-way interaction of task switching, CTI and load ($F < 1$, n.s.), nor the 4-way interaction ($F < 1$, n.s.), were reliable. Other effects of CE load are discussed below. Accuracy was poorer overall at short CTI, with mean overall error rates of 4.7% and 3.4% at short and long CTI, as reflected in a reliable main effect of CTI ($F(1,23) = 14.33$, $p < 0.001$). The main effects of switching for errors was also reliable, but the interaction of switching and CTI was not ($F(1,23) = 13.39$, $p < 0.005$; $F < 1$, n.s., respectively). There were no other reliable effects on errors involving task switching apart from the 2-way interaction with valence, which is discussed below ($F < 1$, n.s. for all).

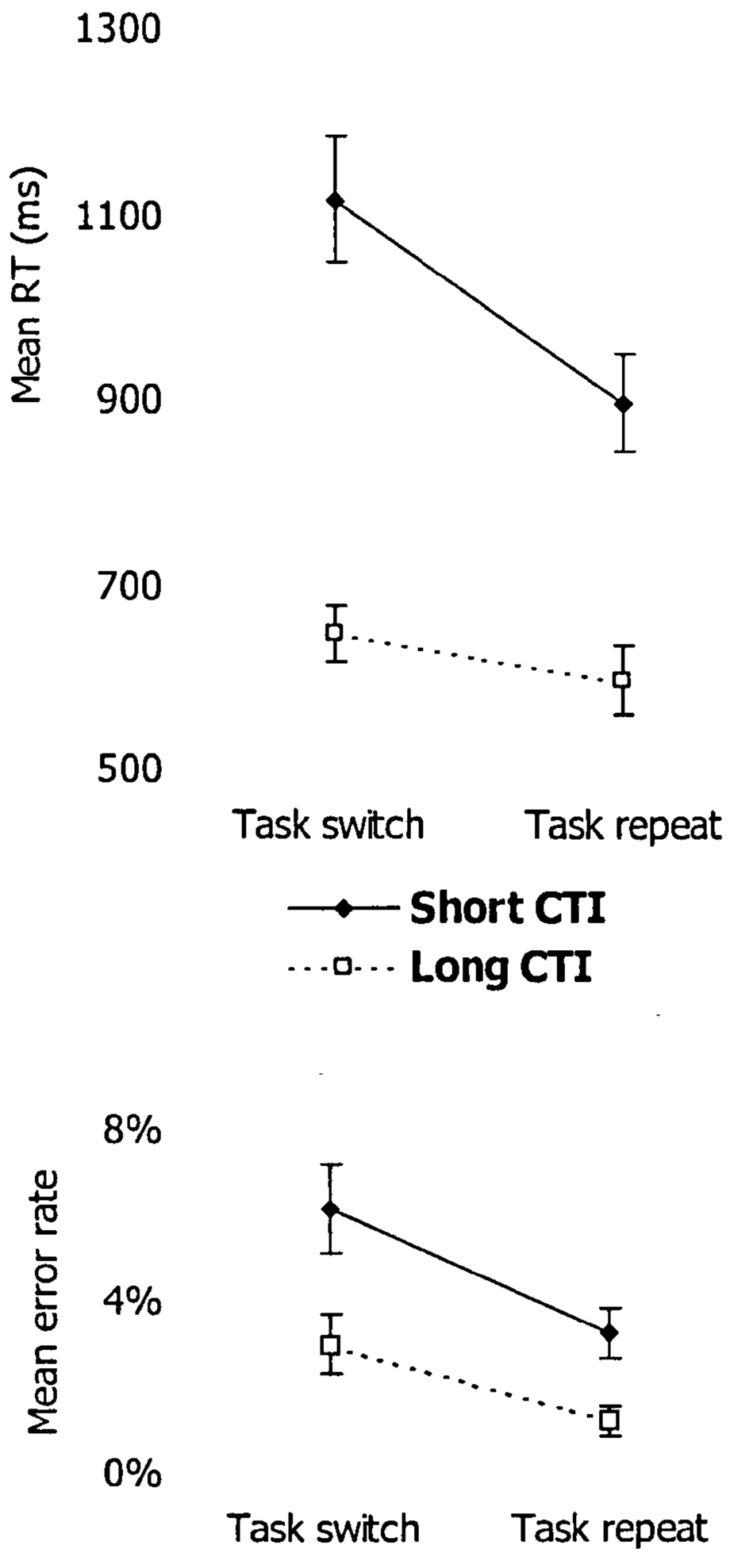

Figure 2.9. The effect of preparation on task switching in Experiment 3, over all concurrent task conditions. Error bars show the standard error of the mean.

CE load effects on task switching

The effects of CE load and stimulus valence on task switching are illustrated in **Figure 2.10.** The pattern of RT switch costs is also summarised in **Table 2.3.** It can be seen that these are, if anything, negative for bivalent trials in the high CE load condition. This was an unexpected pattern of findings. The analysis showed a reliable main effect of secondary task condition for RT ($F(2,46) = 36.80$, $p < 0.001$). Responses were significantly slower in the low CE load than in the single task condition ($F(1,23) = 34.48$, $p < 0.001$), and in the high than in the low CE load condition ($F(1,23) = 23.38$, $p < 0.001$). The error data followed a similar pattern (for main effect of load, $F(2,46) = 13.39$, $p < 0.005$).

	Univalent stimuli	Bivalent congruent stimuli	Bivalent incongruent stimuli
Single task	120ms	117ms	165ms
Low CE load	77ms	96ms	100ms
High CE load	170ms	-31ms	-81ms

Table 2.4. Summary of the RT cost of switching in Experiment 3. Mean data are shown, according to concurrent task condition and stimulus valence.

The main effect of task switching was reliable for RT ($F(2,46) = 9.94$, $p < 0.05$), as was the interaction of switching and load ($F(2,46) = 3.25$, $p < 0.05$, respectively). However, there was also a reliable 3-way interaction of task switching, stimulus valence and load ($F(4,92) = 3.74$, $p < 0.01$). The interaction of secondary task load and CTI on RT was not significant ($F(2,46) = 1.07$, n.s.), and nor was the 4-way interaction ($F < 1$, n.s.). The 3-way interaction did not arise because of an increase in switch cost in any condition, which was what had been predicted, but rather because CE load slowed RT disproportionately on bivalent task repeat trials, thus *decreasing* the switch cost. The simple 3-way interaction effect was reliable for the comparison of the low and the high CE load conditions ($F(2,46) = 3.75$, $p < 0.05$), but not for the the single task versus the low CE load conditions ($F < 1$, n.s.). CE load load influenced the effects of stimulus valence only on task repeat trials. The simple interaction effect of low vs. high CE load and valence was reliable here ($F(2,46) = 9.20$, $p < 0.001$), but not on task switch trials ($F < 1$, n.s.). Further analysis of task repeat trial RT showed that this was because performance differed on univalent and congruent trials (for simple interaction effects of low vs. high CE load and stimulus valence, $F(1,23) = 11.85$, $p < 0.005$), but not on univalent and incongruent trials ($F(1,23) = 16.06$, $p < 0.001$), or on congruent and incongruent trials ($F < 1$, n.s.).

Having established that high CE load was influencing performance selectively on task repeat trials, pairwise comparisons were carried out to clarify the effect in terms of switch costs, remembering that the main differences of interest are between univalent and bivalent trials. In the low CE load condition, there were reliable switch costs or a strong trend for univalent ($F(1,23) = 7.12$, $p < 0.05$), for bivalent-congruent ($F(1,23) = 4.10$, $0.05 < p < 0.1$), and for bivalent-incongruent trials ($F(1,23) = 5.12$, $p < 0.05$). In the high CE load condition, however, switch cost remained significant for univalent trials ($F(1,23) = 10.93$, $p < 0.005$), but there was no sign of a switch cost for bivalent trials (for both types, $F < 1$, n.s.).

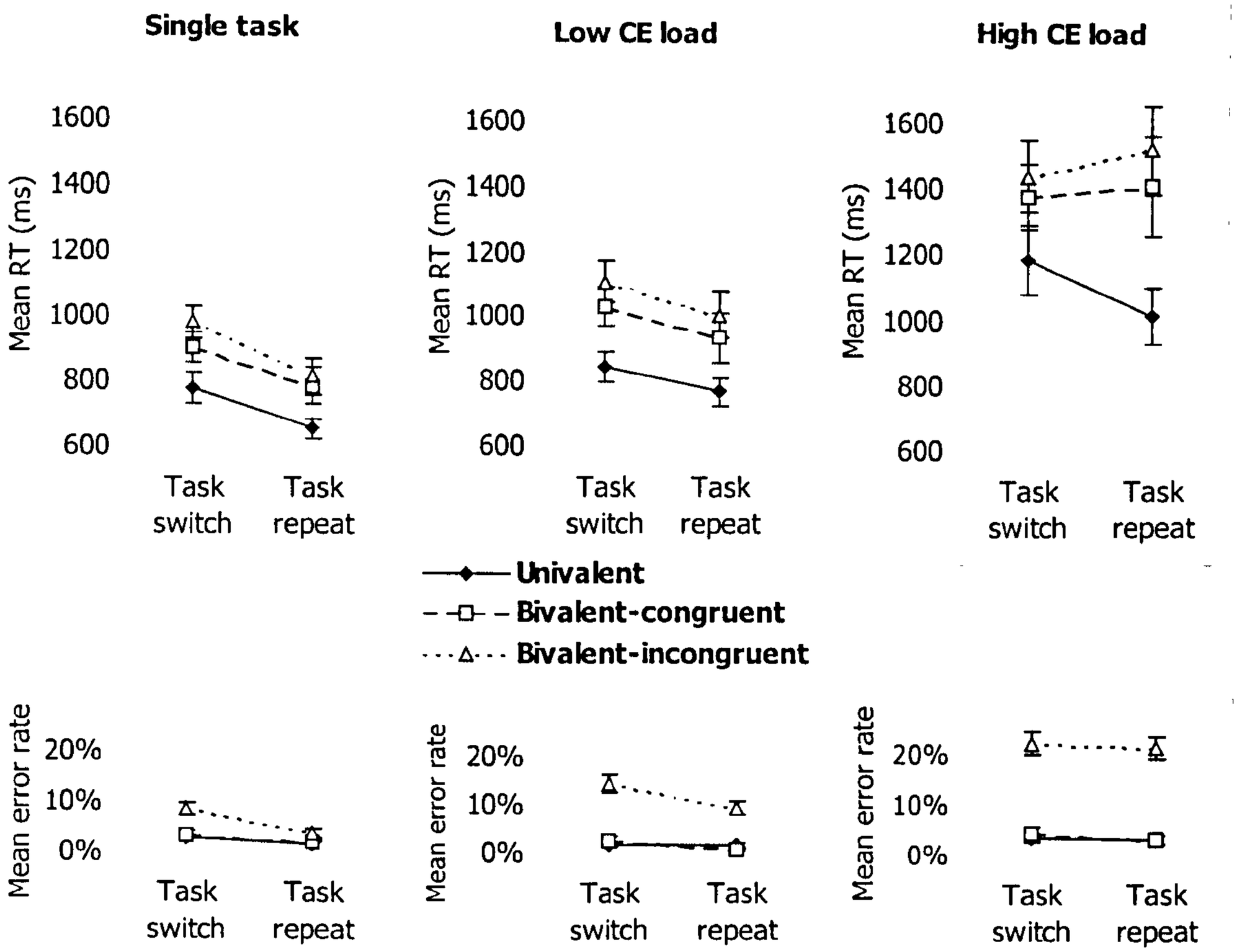

Figure 2.10. Shows the effect of task-irrelevant stimulus attributes on performance on task switch and task repeat trials for Experiment 3. Single task, low CE load and high CE load conditions are given separately to show the effects of concurrent task. Error bars represent standard errors of the mean.

CE load effects for errors

The effects of CE load and stimulus valence on error rates can be seen along with the RT data in **Figure 2.10**. Broadly speaking, the error data mirror the RT data, in that the switch cost (seen mainly on incongruent trials) is less in the high CE load condition than in the single task condition. There had been no specific predictions here.

There were reliable interactions for errors of task switching with stimulus valence ($F(2,46) = 3.37$, $p < 0.05$) and of secondary task load with valence ($F(4,92) = 30.06$, $p < 0.001$), although the 3-way interaction of these factors was not significant ($F < 1$, n.s.). There was also a reliable main effect of valence ($F(2,46) = 130.55$, $p < 0.001$). The interaction of stimulus valence and task switching arose because the simple main effect of switching was reliable on congruent ($F(1,23) = 5.84$, $p < 0.05$) and incongruent ($F(1,23) = 7.43$, $p < 0.05$) bivalent trials, but not on univalent trials

(F(1,23) = 1.49, n.s.).

The interaction of secondary task load and valence arose because CE load affected error rates only where there was interference between tasks, on bivalent trials. In the low CE load condition, the simple interaction effect of load and stimulus valence was reliable (F(2,46) = 26.12, $p < 0.001$). This was because there were more errors on incongruent trials than in the single task condition, as reflected in a reilable simple main effect of load (F(1,23) = 29.71, $p < 0.001$); this was not reliable for univalent (F(1,23) = 1.04, n.s.) or for bivalent-congruent trials (F(1,23) = 1.75, n.s.). The simple interaction effect of load and stimulus valence was also significant in the high CE load condition (F(2,46) = 16.92, $p < 0.001$), where the simple main effect of high versus low CE load was reliable for incongruent (F(1,23) = 28.71, $p < 0.001$) and for congruent (F(1,23) = 7.96, $p < 0.05$) trials. For univalent trials, load did not affect error rates significantly (F(1,23) = 3.20, $0.05 < p < 0.1$). Thus low and high CE load increased error rates on incongruent trials, and high CE load increased them also on congruent trials.

Effects not involving task switching

The interaction of CTI and stimulus valence for RT was reliable, as in the single task condition (F(2,46) = 17.34, $p < 0.001$). However, the interaction of CTI and load wa not significant (F(2,46) = 1.07, n.s.), and nor was the 3-way interaction of CTI, load and valence ($F < 1$, n.s.). There was a similar pattern of performance for errors, with a reliable interaction of CTI with valence (F(2,46) = 5.75, $p < 0.01$), but no significant effect on this of CE load (3-way interaction with secondary task load, $F < 1$, n.s.). The interaction of preparation and valence effects on RT and on errors are discussed further, below, when differences from Experiment 2 are considered.

Comment on individual performance

There was a high rate of errors in this study on incongruent trials, in particular in the high CE load condition (21% overall, compared with 5.3% in Expt. 2). This led to concern that participants were frequently not responding according to the correct task rule, and that this would mask effects involving task switching on RT, since to measure switch costs participants must respond using the correct task rule most of the time. Individual data were examined to see whether some participants were particularly inaccurate, and whether their exclusion from the analysis would reduce the noise in

the data. Although participants were instructed to respond as quickly as possible whilst still being accurate, there was substantial variation in overall error rates. One person did stand out, making 11.6% errors overall, whilst most participants made between 3% and 6%, with a few between 7% and 9%. The single task data were reanalysed excluding this participant, but this did not alter the pattern of findings, and in particular the interaction of task switching and valence was still not reliable ($F < 1$, n.s.). There was a more marked trend towards a 3-way interaction of task switching with both CTI and stimulus valence, but this was still not statistically significant ($F(2,46) = 2.03$, n.s.). This relationship will receive further consideration in Chapter 4.

Proportional effects of load?

The basic methodology so far has been based on additive factors logic, where an interactive effect of two factors on response times is taken as evidence that they both affect the same stage of processing (Sternberg 1969). Aside from any general debate about such a method, in any experiment using a dual task manipulation, there is the additional problem that there are two different kinds of mechanism that could potentially explain an interactive effect of a secondary task with another factor. The first is the obvious, and unproblematic, possibility that performance of the secondary task selectively engages the process affected by the other manipulation. It is, however, also possible that the secondary task could be affecting some general resource leading to a general proportional slowing of response times in all conditions. Clearly the latter can only explain interactions where the condition disproportionately affected by the secondary task is that with the longest baseline RT. The apparent effect in this study of high CE load on task switching and valence effects does not fit this pattern, since it was task repeat trials that were particularly affected by the secondary task, *reducing* the difference in RT between them and task switch trials. It was therefore concluded that analysing secondary task effects in terms of proportions could not affect the conclusions from this study in terms of effects on task switching.

Summary

Analysis of performance under single task conditions showed a reduction in task switch cost with preparation, as found in previous studies. Preparation for a switch was therefore clearly effective with endogenous task cues and randomly varying cue-target interval. Contrary to expectation there was no reliable increase in RT switch cost for

bivalent compared with univalent stimuli. However, a trend towards such an effect for errors is noted on incongruent trials. Interference from the other task again slowed RT (as opposed to switching), and this effect was more marked at short CTI. A similar effect was found for errors.

There was no evidence to support the prediction that preparation for a task switch, using endogenous cues, would be impaired by CE load. Switch cost was not increased at short CTI by either concurrent task. The other possible predicted effect of high CE load, that preparation for a switch would not be effective, so long CTI switch costs would be increased, was difficult to evaluate meaningfully since high CE load unexpectedly *reduced* switch costs. This was because task repeat trial performance was slowed by load in the presence of task-irrelevant information. Reliable switch costs were maintained only on univalent trials in the high CE load condition.

Comparison of performance in Experiments 2 and 3

Differences between Experiment 3 and Experiment 2 were also explored for the single task and for the experiment as a whole. Two ANOVAs were carried out using the same repeated measures factors as above, with the additional between subjects factor of group (Experiment 2/ Experiment 3). Only reliable differences between the two studies are discussed here, as overall performance was not directly of interest. With the exception of effects that include CE load, the focus is also on the comparison of baseline performance in the two studies.

Comparing baseline performance

Effects of preparation on task switching

In the single task condition, there was a significant difference between exogenous and endogenous task cueing with respect to preparation and the time cost of task switching. This is illustrated in **Figure 2.11**, with error data for comparison. It can be seen that the RT switch cost was greater at short CTI with endogenous than with exogenous task cueing.

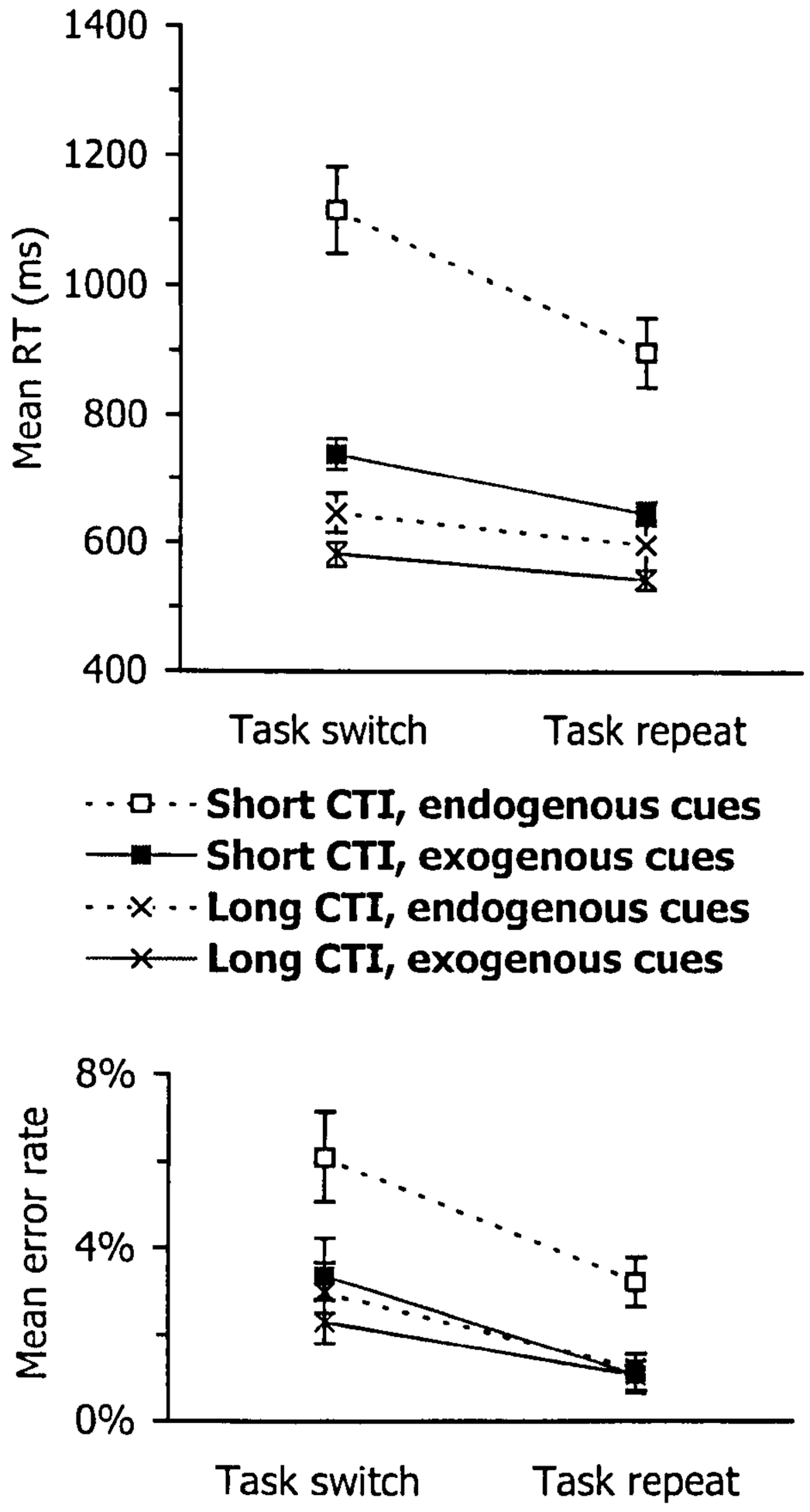

Figure 2.11. Preparation for a switch with endogenous and with exogenous task cueing. This compares performance in the present study with that in Experiment 2. Mean RT and error proportions as a percentage are shown. Error bars represent the standard error of the mean.

The main effect of group on RT, and the interaction of experiment group and task switching were both reliable ($F(1,46) = 15.96$, $p < 0.001$, and $F(1,46) = 7.68$, $p < 0.01$). There was also a significant interaction of group with CTI, and a 3-way interaction of both these factors with task switching ($F(1,46) = 53.31$, $p < 0.001$, and $F(1,46) = 6.72$, $p < 0.05$). This resulted from a difference in switch costs between Experiment 3 and Experiment 2 at short CTI (for simple interaction effect of group and task switching, $F(1,46) = 10.90$, $p < 0.005$). The difference between the groups at short CTI was reliable for task switch trials (for simple main effect, $F(1,46) = 28.94$, $p < 0.001$), and for task repeat trials ($F(1,46) = 19.77$, $p < 0.001$). Performance was not reliably different in the two experiments at long CTI (for simple interaction effect of

group and task switching, $F(1,46) < 1$, n.s.; for simple main effect of group, $F(1,46) = 2.71$, n.s.).

With respect to errors, there a reliable main effect of experiment group ($F(1,46) = 7.80$, $p < 0.01$), and interaction of group with CTI ($F(1,46) = 5.43$, $p < 0.05$). Overall, there were more errors in Experiment 3 than in Experiment 2, particularly at short CTI, but as there was a higher order interaction with stimulus valence, this is considered further below. There were no other reliable differences in error rates between the two studies.

Task switching and interference

Switch costs in Experiment 3 overall, by stimulus valence, were 121ms for univalent trials, 117ms for bivalent-congruent trials, and 155ms for bivalent-incongruent trials. Comparable switch costs in Experiment 2 were 31ms, 73ms and 92ms, respectively. As described above, interference between tasks did not increase the switch cost reliably in Experiment 3 as it had in Experiment 2. However, the quantitative comparison between the two studies in this respect was not reliable (for 3-way interaction of task switching and valence with group, and for 4-way interaction, $F < 1$, n.s.; the higher order interaction with secondary task load is discussed below).

There was also no reliable effect of stimulus valence on the error switch cost, and again, there had been such an effect in Experiment 2. However, as for RT, the comparison between the two groups was not reliable in this respect (for interaction of group with task switching and valence, $F < 1$, n.s.). It should also be noted that over Experiment 3 as a whole, there *was* a reliable error switch cost for bivalent stimuli.

Other stimulus valence effects

The relationship between preparation and stimulus valence, independent of task switching, is illustrated in **Figure 2.12** for both Experiment 2 and Experiment 3. There was a significant interaction of experiment group with valence ($F(2,92) = 3.48$, $p < 0.05$), and a 3-way interaction of group with CTI and valence ($F(2,92) = 3.13$, $p < 0.05$). At short CTI only, there was more RT slowing from incongruent irrelevant attributes with endogenous (Experiment 3) than with exogenous (Experiment 2) task cues. The simple interaction effect of experiment group and valence was reliable only at short CTI ($F(2,92) = 4.03$, $p < 0.05$; at long CTI, $F(2,92) = 1.85$, n.s.).

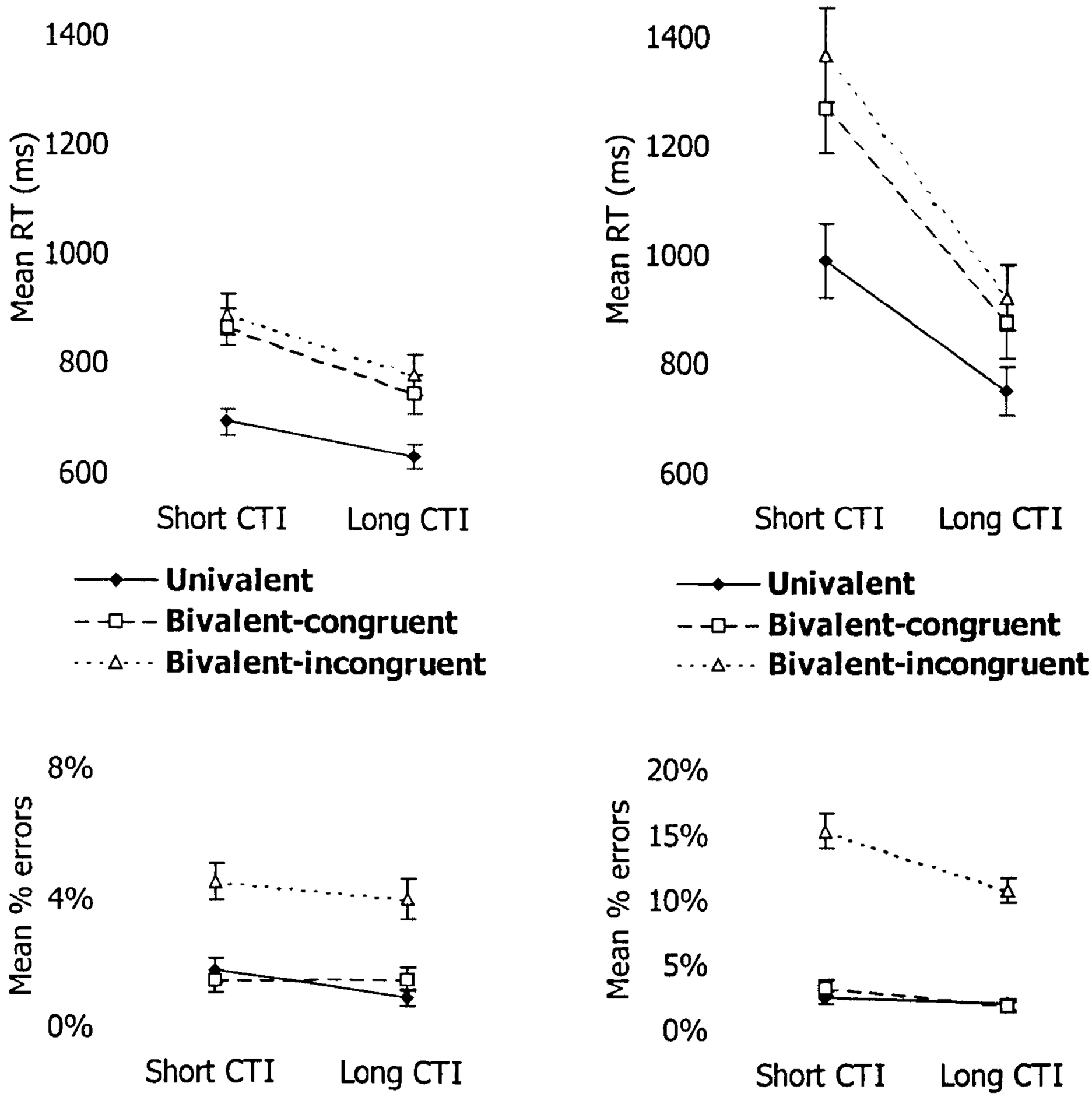

Figure 2.12. The influence of preparation on valence effects in Experiments 2 (exogenous task cueing) and 3 (endogenous task cueing). The data shown are for the single task condition only. Error bars represent the standard error of the mean.

Although RT was reliably slower in Experiment 3 than in Experiment 2 at short CTI for all stimulus types, the only significant difference was between incongruent and univalent trials. The simple interaction effect of group and valence was reliable for univalent versus congruent trials ($F(1,46) = 3.44$, $p < 0.1$), and for univalent versus incongruent trials ($F(1,46) = 6.63$, $p < 0.05$; for congruent versus incongruent $F < 1$, n.s.). The simple main effects of experiment group, i.e. endogenous or exogenous task cueing, at short CTI, was relialbe for all trial types (for univalent trials $F(1,46) = 27.72$, for congruent trials $F(1,46) = 23.55$, for incongruent trials $F(1,46) = 23.98$, $p <$

0.001 for all). For errors, although there was a reliable interaction of CTI and valence in Experiment 3, but not in Experiment 2, the 3-way interaction of experiment group with CTI and valence was not reliable ($F < 1$, n.s.).

Overall performance and effects of CE load

Effects of preparation on task switching

The differences between exogenous and endogenous cueing in terms of preparation and switch cost applied across all secondary task conditions. For Experiment 3, mean task switch and task repeat RTs were 1283ms and 1132ms at short CTI, and 855ms and 843ms at long CTI. Equivalent mean RTs in Experiment 2 were 851ms and 778ms at short CTI, and 738ms and 698ms at long CTI. Comparing Experiments 2 and 3, the interactions of group with task switching and CTI, and of group with CTI, were both reliable overall ($F(1,23) = 5.25$, $p < 0.05$; $F(1,23) = 64.26$, $p < 0.001$, respectively). These effects followed the same pattern as for the single task conditions, as switch cost was greater in Experiment 3 only at short CTI (for simple interaction effect of group and task switching, $F(1,23) = 5.82$, $p < 0.05$; for long CTI, $F < 1$, n.s.). Overall RT was slower at short CTI both on task switch and on task repeat trials in Experiment 3, although the difference was larger for the former (for simple main effect of group, $F(1,23) = 26.63$, $p < 0.001$ for task switch trials, and $F(1,23) = 17.90$, $p < 0.001$ for task repeat trials). The only (minor) difference from the analysis of the single task conditions alone was that, although switch costs did not differ between the studies at long CTI, overall RT was slower at long CTI in Experiment 3 than Experiment 2, with means 815ms and 718ms (for simple main effect of group, $F(1,23) = 4.36$, $p < 0.05$; this was not reliable for the single task alone). There was a main effect of experiment group on error rates ($F(1,46) = 45.56$, $p < 0.001$), but no reliable differences between the two studies involving task switching.

Effects of CE load on switching for RT

The effect of CE load on task switching demonstrated in Experiment 3 was not found in Experiment 2, with exogenous task cueing. Details of the findings for experiment 3 have been discussed above, and are illustrated in **Figure 2.10**. Mean RTs and error rates are given in **Table 2.2** for the high CE load condition of Experiment 2. In that study, the increase in RT switch cost for bivalent stimuli was reliable overall, and not reliably affected by load. There had been some loss of switch costs with high load, but

these were dependent upon CTI (and not statistically significant). The difference between the two studies was confirmed by a reliable 4-way interaction for RT of experiment group with task switching, stimulus valence and load condition ($F(4,92) = 2.43$, $p < 0.05$). The 2-way interaction of experiment group with valence was also significant, reflecting a general increase in interference effects on RT, as had been found in Experiment 2 ($F(2,46) = 5.71$, $p < 0.005$). There were no other reliable interactions involving experiment group and secondary task condition (for interaction of group with load, $F(2,46) = 2.68$, $p < 0.1$; for group with load and switching, $F(2,46) = 1.04$, n.s.; for all others, $F < 1$, n.s).

CE load effect on errors

Mean error rates on incongruent trials for single task, low and high CE load were 5.9%, 11.6% and 21.2% respectively in Experiment 3, and 3.7%, 3.7% and 5.3% in Experiment 2. The overall effect of secondary task load on errors was greater in Experiment 3 than in Experiment 2, particularly on incongruent trials, as reflected in reliable interactions of group with secondary task condition ($F(2,46) = 17.46$, $p < 0.001$), and of group with secondary task condition and valence ($F(4,92) = 18.76$, $p < 0.001$). This effect was significant in the comparisons both of the single task and the low CE load conditions ($F(2,46) = 13.80$, $p < 0.001$), and of the low and the high CE load ($F(2,46) = 8.75$, $p < 0.001$). In both cases, there were more errors on incongruent trials in Experiment 3 than in Experiment 2 with increasing CE load (for simple main effects of group and load on incongruent trials, $F(1,23) = 15.61$, $p < 0.001$; $F(1,23) = 14.37$, $p < 0.001$, respectively). For congruent and univalent trials, the differences were not reliable (for simple main effects of group and load on congruent trials, $F(1,23) = 1.14$, n.s. for single task vs. low CE load; $F(1,23) = 3.36$, $p < 0.1$ for low vs. high CE load; for univalent trials, $F < 1$, n.s. for both comparisons). All other effects on errors involving CE load were non-significant ($F < 1$, n.s.).

Preparation and stimulus valence

There were differences between the two studies with respect to preparation and stimulus valence, for RT and errors, that essentially mirrored the findings already described for the single task condition. The data are summarised in **Table 2.5**. The 3-way interaction of group with CTI and valence was reliable ($F(2,46) = 6.93$, $p < 0.005$), because at short CTI there was more RT slowing for bivalent stimuli in Experiment 3 than in Experiment 2.

	Univalent stimuli		Bivalent congruent stimuli		Bivalent incongruent stimuli	
EXPERIMENT 2	RT (ms)	% errors	RT (ms)	% errors	RT (ms)	% errors
Long CTI	629	0.9	745	1.5	780	4.0
Short CTI	692	1.7	865	1.4	887	4.5
EXPERIMENT 3						
Long CTI	750	1.8	876	1.6	921	10.6
Short CTI	989	2.3	1268	3.1	1365	15.2

Table 2.5. Shows the mean RTs and error rates in Experiments 2 and 3 according to CTI and task-interference (stimulus valence).

For errors, there were reliable 2-way interactions of experiment group with both CTI ($F(1,46) = 8.46$, $p < 0.01$) and stimulus valence ($F(2,92) = 52.39$, $p < 0.001$), and a 3-way interaction of group with both CTI and valence ($F(2,92) = 4.63$, $p < 0.05$).

Summary of differences between exogenous and endogenous cueing

The comparison of baseline performance conditions showed differences between Experiment 2, where task cues were exogenous, and Experiment 3, where they were endogenous. The most important of these was that switch cost with endogenous cues was greater at short CTI than with exogenous cues. This was also true when all secondary task conditions were considered together, and preparation for a task switch appeared to be equally unaffected by CE load in both studies. In both cases, preparation reduced switch cost, and the remaining or residual cost was not significantly different with the two types of task cueing. However, when univalent trials were analysed separately, evidence of a residual cost was found in Experiment 3. No evidence of a residual cost had been found in Experiment 2.

The formal comparison of the two studies confirmed the finding that CE load affected task switching with endogenous but not with exogenous task cueing, although the pattern of this effect was unexpected. In Experiment 3, where RT switch cost was abolished by high CE load for bivalent stimuli, switching performance was reliably different from that in Experiment 2.

However, the interaction of preparation and stimulus valence for RT (as opposed to switching) was significantly more marked than it had been with exogenous task cues. The effect of interference from the other task on RT was again found to be greater at short CTI. There was a parallel effect for errors only in Experiment 3, for all secondary

task conditions together. As they did not involve task switching, these effects had not been predicted. Error rates were higher in Experiment 3 than in Experiment 2, particularly on incongruent trials with increasing CE load, but this was not affected by task switching.

Discussion of Experiment 3

Task switching with endogenous cues

In order to test the predictions about the effects of CE load on preparation for a switch, this study needed to establish that effective preparation for a switch is possible with endogenous task cues. This was clearly demonstrated. As predicted, the time cost of task switching was still reduced by preparation when participants were instructed on each trial to 'switch' or to 'stay' rather than explicitly prompted as to which task to perform. In previous studies of preparation for a switch, there had been *some* external cue as to which task to perform (Shaffer 1965; Rogers and Monsell 1995; Meiran 1996). This result also made it clear that preparation is not necessarily prevented by random variation of the cue-target interval within blocks of trials (see Rogers and Monsell 1995).

An unexpected finding was that the time cost of task switching in this study was not reliably increased for bivalent, compared with univalent, trials. No particular prediction had been made about this for endogenous task cues. Since the effects of interference and preparation on switching have so far been found to be independent, changing the cueing method was not expected to alter the former. In support of this, it is noted that this difference between the two studies was not statistically significant in the comparative analysis. This finding seems likely to have been the result of less accurate performance here, since there was a trend towards an effect of stimulus valence on the error cost of switching in the single task condition.

Effects of CE load on task switching

Task switching and preparation

The main reason for this study was to determine whether the central executive is involved in the control of switching only when the switch itself requires endogenous control. Two possible effects of central executive load on task switching were

predicted, as for Experiment 2, but again there was no good evidence for either. There had been two alternative hypotheses about CE involvement in preparation for a switch. The first possibility assumed that when cues instruct participants to switch, but do not explicitly cue the relevant task, the endogenous component of switching requires the central executive. The endogenous component of switching is defined as the part of processing that can be carried out before the arrival of the stimulus if there is time to prepare, but contributes to the time cost of a switch if there is not (as at short CTI) (Meiran 1996). It was therefore predicted that switch cost at short CTI would be increased by CE load, because the endogenous component of switching was taking longer. This was clearly not the case in either secondary task condition.

The second possibility was based on Rogers and Monsell's (1995) claim that executive involvment in switching is under strategic control, again, in the absence of strong external task cues. It therefore may not be the endogenous component of switching in itself that is executive in nature. On this view, it is the ability strategically to carry out this advance processing that is the executive contribution to a task switch. The prediction was that, with a central executive load, instead of the endogenous component taking longer, participants might opt not to prepare for a switch. Switch cost at long CTI would therefore be increased and similar to that at short CTI. There was no specific evidence that such strategic preparation for a switch was impaired by load. However, this prediction could not be assessed fully as CE load had another, unexpected effect on task switching performance, which led to a reduction in switch costs. The results did not indicate clearly either a preservation or an absence of an effect of preparation on univalent trials, for which switch costs were still found.

Of the three main predictions of this study, the second possibility, that the central executive is required to prepare for a task switch with endogenous cues, has therefore been ruled out, with the above proviso about strategic preparation. The third prediction concerned whether or not the manipulation of cueing would affect the endogenous component of switching. This will be evaluated in the General Discussion, where the two studies are compared, and the implications for models of task switching and executive control considered. The first prediction, that CE load would not affect preparation for a switch, was largely supported.

Task switching and interference

Despite these findings, switching was clearly influenced by CE load in this experiment.

The effect was just not dependent on preparation. What might be the explanation for the unexpected *reduction* in switch costs with concurrent load? It was found specifically in the comparison between the low and the high load conditions, indicating an effect of central executive load, and only affected bivalent trials. It appeared to result from a disproportionate slowing of RT on task repeat trials.

Could this have been due simply to a breakdown in performance accuracy with the high CE load secondary task to a level where the effect of task switching was no longer reliable? Errors were made on incongruent trials about 22% of the time, suggesting that participants may quite often have been failing to respond according to the correct task rule, as specified in the instructional cue. Perhaps they simply could not remember which task they had performed on the last trial, so switching performance was maintained reasonably well only on univalent trials, where the stimulus unambiguously indicates the task to be performed. If participants guessed the task rule on a significant proportion of bivalent trials, this might reduce the apparent effect of stimulus valence on RT switch cost, as well as increasing the noise in the data. This could imply a specific effect on working memory, presumably (because it was not found in the low CE load condition) the central executive. However, it is not clear in this situation why the effect of high CE load was specific to *task repeat* trial performance. This is not easily accounted for by a failure to comply with task instructions. Could the latter have been explained by a different speed-accuracy trade-off under conditions of central executive load, with participants being slowed by the presence of task-irrelevant stimulus attributes when the task repeated, but making more mistakes when the task switched? In analysis of the data, an increase in switch trial errors on bivalent trials was noted for the experiment as a whole, and this had not been reliable in the single task condition baseline. However, inspection of the data revealed that the trend in this direction was more marked in the low than in the high CE load condition (although not statistically significant for either[1]). As the effect of concurrent load on RT switch cost did not occur with low CE load, this makes such an explanation unlikely. It seems necessary therefore to consider the possibility that task control may not affect performance exclusively when the task switches. This will be explored in the General Discussion, after the comparison between exogenous and

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[1] For the interaction of task switching and stimulus valence for errors in the low CE load condition, $F(2,46) = 3.26$, $0.025 < p < 0.05$; for the high CE load condition, $F(2,46) = 2.49$, $0.05 < p < 0.1$ (adjusted $\alpha = 0.025$).

endogenous task cueing has been considered.

Working memory and task cueing

Given the present finding, that CE load reduces switch cost for bivalent stimulus trials, the question arises of why studies using the list-method have not demonstrated any similar effect? The details of these experiments have already been described in Chapter 1. To recap briefly, Baddeley et al found that concurrent performance of a task with high CE load affected the speed of baseline addition and subtraction more than it affected the cost of task alternation (Baddeley, Chincotta et al. Submitted). Because the present study did not include a pure-task condition, however, it is difficult to compare these findings. Also, the effect found here, being specific to task repetition trials in mixed blocks, would have no parallel in an alternating list experiment, where in mixed blocks all trials are switch trials.

A point that also deserves some consideration is to what extent the 'high load' task used here is likely to have loaded the central executive effectively. The established random generation task was not chosed for the present experiments, mainly because of the claim that its executive component result from switching (between retrieval strategies, see Baddeley 1996). The use of serial mental arithmetic tasks in the present study was suggested by evidence, from various experiments, of CE involvement in mental arithmetic. Logie et al (1994) found evidence that the CE is involved in calculation, particularly in producing approximately correct answers. Other investigations have suggested a role for the CE in reasoning tasks (Gilhooly, Logie et al. 1993). An important factor affecting the difficulty of mental arithmetic problems in general, and the role of working memory in solving them in some studies, is the number of carries necessary in a particular task (Hitch 1978). The 'adding 7's' task in the present study was not particularly difficult in this sense, requiring a single carry for every other operation. However, in Logie et al's experiment, the number of carries did not interact with the effect of random generation on addition, so this was unlikely to be a barrier to effective CE load by the task used here (Logie, Gilhooly et al. 1994). Indirect support for a role of the CE in serial mental arithmetic also comes from a study of chess by Holding, which found that counting backwards in threes led to the selection of poorer quality moves (Holding 1989). Although he attributed this to articulatory suppression by this secondary task, this interpretation was challenged Robbins et al, in another study of chess (Robbins, Anderson et al. 1996). They used the more

conventional secondary tasks of random letter generation for the CE, compared with articulatory suppression, and findings suggested that it was the CE that was responsible for impaired move selection. The authors inferred from this that Holding et al's counting task placed substantial demand upon the CE.

Phonological loop effects

The effect of secondary task performance on task switching in the present study was specific to the high CE load condition, and was therefore not an effect of articulatory suppression. The studies of switching using mental arithmetic referred to above, however, have found that the alternation cost was increased by (certain) articulatory suppression tasks. This only occurred where task cues are absent, so switching was cued endogenously (Baddeley, Chincotta et al. Submitted). Why, then, did the present study not find any evidence of phonological loop effects on switching? Where tasks alternate and stimuli for both tasks are indistinguishable, the control of which task rule to apply is entirely endogenous (in the sense used here), but in addition, participants have to determine *when* to switch. In the present paradigm, they are instructed when to switch but must determine the task rule themselves. This difference might explain the apparently discrepant finding, if the phonological loop were important in timing a switch, although there is no evidence to support this. It is also possible that the phonological loop effect in the task alternation experiments was not on the task switch event specifically, but on the mixing of two tasks within the same block. This presumably requires participants to form some kind of on-line representation of the task rules, which might be verbally coded. In the present study, such a 'mixing cost' would not have been detectable as pure task blocks of trials were not included. However, this seems unlikely, since verbal coding did not seem to be involved in on-line memory for tasks in experiments by Allport and Styles (1990).

What implications do the present findings have for models of task switching and for a more general understanding of executive control? In order to answer this question, they must be considered in the context of the findings of the earlier studies in this Chapter.

General Discussion

Endogenous task cueing

The use of endogenous instead of exogenous task cues in Experiment 3 affected performance in a number of important ways. One of these effects involved preparation for a task switch. The likely underlying processes and theoretical implications of this will be considered first. The effects of task cueing that interacted with concurrent load will then be discussed.

Cueing effects on preparation for a switch

One of the possible patterns of results predicted for Experiment 3 was that the control of task switching would be influenced by the change in cueing method, although this would be unaffected by central executive load. The comparison of performance under single task conditions in Experiments 2 and 3 supported this prediction. It was shown that task switching performance was different when task cues did not directly trigger the new task rule but simply instructed participants to 'switch'. The time cost of switching was greater at the short cue-target interval, suggesting that a switch took longer, but only when there was little time to prepare.

Could this have been due to an effect on the processing of the cue words themselves? Slower cue processing could have explained the longer overall response times at short CTI in Experiment 3, but there is no reason to expect this to take longer on task switch than on task repeat trials. The cues were similar in appearance, and the cue word repeated when the task repeated, and changed when the task changed, in both studies. The only exception to this was on the first trial that the task repeated in Exeriment 3, where the cue again changed (from 'SWITCH' to 'STAY'). The apparent effect of endogenous cueing on preparation for a switch cannot have been due to a simple cue word repetition/ switching effect, since this would lead one to predict a *smaller* switch cost in Experiment 3, due to cues changing on many task repetition trials, i.e. on the first repetition after a switch. The disproportionate effect of the type of cues on task switch trials suggests that with cue processing subtracted out, there remained an effect of cue type on processes not involved when the task repeats.

Implications for models of task switching

This finding suggests that the component of the switch cost described as 'endogenous' by Rogers and Monsell did indeed take longer with 'endogenous' task cues. This process can be engaged before the arrival of the relevant stimulus, but contributes to the switch cost where there is no preparation (Rogers and Monsell 1995; Meiran 1996). What is the nature of the increased demand on the endogenous control of switching with endogenous task cueing? Firstly, it cannot relate directly to the need to remember the task rule from the previous trial in order to follow the instruction 'switch' or 'stay', since this requirement applied on task repeat as well as on task switch trials. Secondly, as in this study the cues indicated *when* to switch, this control must relate to the retrieval of the correct task rule, to its activation once retrieved, or to the inhibition of the alternative rule. If rule retrieval is involved, then with endogenous task cueing an additional processing step might be required where participants had to recall which task was performed on the *previous* trial. This seems somewhat less parsimonious than the alternative suggestion, that rule retrieval might simply be faster with more direct or 'stronger' external cueing. The third possibility, an effect of cueing method on the inhibition of a competing task rule when preparing for the new task, seems less likely, because there was no effect of stimulus valence on the interaction between task cueing and preparation for a switch. These possibilities could be distinguished by further experimentation, for example, the rate of preparation could be examined by exploring switching performance with different cue types over a range of CTIs.

Rogers and Monsell's (1995) model of task switching can only account for this extra processing in terms of an increased requirement for executive, or endogenous, control, which effectively just restates the finding. Meiran's original (1996) 2-component model was similar in this respect to that of Rogers and Monsell. As explained in Chapter 1, Meiran and coworkers have more recently identified the preparatory component of switching with the reconfiguration of stimulus sets, or shifting attention to the relevant stimulus dimension (see Meiran 1999; Meiran In press). However, without more detailed specification, it is unclear why reconfiguration of stimulus sets should take longer with endogenous task cues.

Rubenstein et al's model can perhaps provide a testable explanation. The model describes two executive processes involved in switching, goal shifting and rule activation (Rubenstein, Meyer et al. In press). These have been distinguished by interactions with different variables (see Chapter 1). Goal shifting involves inserting

and deleting (task) goals in 'declarative working memory', and it is suggested that this can take place before the stimulus arrives, but is also affected by task cueing. One can hypothesise that with exogenous task cues this stage is faster. If this were the case then other manipulations affecting this stage will increase the difference in switching performance between exogenous and endogenous task cues. The strength of exogenous cueing (and consequently the amount of endogenous control) could be varied, for example, by comparing practiced or prepotent task cues with more novel ones. On the other hand, manipulations affecting rule activation, such as the presence of interference between tasks, should not interact with the effects of task cueing. This is consistent with the findings of Experiment 3 in that, as in Experiment 2, stimulus valence did not affect switching *more* at short CTI. However, other effects of task-irrelevant information were less clear-cut with endogenous cueing. None of these models make predictions about other aspects of task control, so cannot speak to the interaction of task cueing and interference other than when a switch is involved. These issues are considered further below.

Residual switch cost

It is worth noting at this point that, although performance at long CTI did not differ reliably between Experiments 2 and 3, there was some difference between the two studies in the *pattern* of residual switch costs. In Experiment 2, switch cost at long CTI was reliable overall, but for univalent trials it was 19ms, and not significant. In Experiment 3, residual cost was a reliable 62ms for univalent trials. In a neuropsychological study of task switching, Rogers et al varied the strength of exogenous task cueing, and (although the relevant statistics were not given) there did not appear to be a residual switch cost in the healthy control groups with weaker external cues. If the cueing method affects the residual cost of task switching this is contrary to all three models discussed above. The only account that could accommodate such a finding is that of de Jong, where residual costs are claimed to result from a failure to prepare for a switch on a proportion of switch trials, rather than incomplete preparation on all trials (DeJong, Berendsen et al. 1999). The possibility was raised that endogenous task cues might make participants less likely to 'opt' to prepare, and a weak effect of this nature might be reflected in greater residual costs with endogenous cues, if de Jong's account is correct. An opportunity to replicate this apparent difference, and further consideration of these issues, will be reported in Chapter 4.

Endogenous control and switching

The finding that the 'endogenous' process involved in task switching took longer when 'endogenous' task cues were used also has implications for task control beyond task switching. Rogers and Monsell (1995) described the part of the switch cost that is abolished by preparation as the 'endogenous component', on the a priori basis that preparing a task set or rule before the stimulus arrives requires control beyond that required for task performance itself. Allport et al (1994) had based their account of switch costs as 'task set inertia' around their finding that they appeared to be affected by processing on previous trials and *not* by preparation. Both Rogers and Monsell and Meiran (1996) presumptively linked preparation for a switch to an executive mechanism by applying the Norman and Shallice (1980) model of the control of action, in which the SAS is involved in 'endogenous' control, by definition. The present data suggest that there is more than a semantic link between the two terms; there is now an experimental link. Weaker external task cues, where participants must themselves determine what task rule to apply, and apply it, do place an increased demand on the 'endogenous' component of task switching.

But do the studies reported here provide any evidence that this endogenous control is executive, in the sense of being carried out by a common mechanism that is also responsible for other cognitive control? In order to answer this question, the use of endogenous task cueing must be linked to changes in aspects of task control unrelated to switching. In particular, the effect that using endogenous task cues had on the control of interference between tasks requires further consideration.

Task cueing, interference and executive control

Another aspect of performance clearly affected by the use of endogenous, as opposed to exogenous, task cues was the influence of irrelevant information from the other task. Differences were found between Experiments 2 and 3 in the effect CE load had on task switching, which affected bivalent trials in Experiment 3 only. This has been considered already in detail (see Discussion of Experiment 3).

There was another difference between the two studies that was not dependent on task switching and repetition. Regardless of whether the task switched or repeated, the slowing of response times attributable to interference between tasks was more marked in Experiment 3 than in Experiment 2. Congruent as well as incongruent bivalent trials

were affected, suggesting an effect at the level of the task set, rule, or stimulus category. For RT, this occurred only in the absence of time to prepare, when CTI was short. However, there was an increase in the error rate on incongruent trials at long as well as at short CTI with endogenous cueing. As explained earlier, this is likely also to represent task-set cueing, despite the absence of an effect on congruent trials, since responding according to the wrong task rule still leads to a correct response if the task-irrelevant attribute is congruent. In Experiment 2, it was not possible to draw conclusions about task control from a similar finding, since the univalent and bivalent stimuli differed visually different. However, these factors applied equally in Experiment 3. This difference between the two experiments in the effect of stimulus valence cannot be therefore be explained by an effect of task cueing on the visual encoding of irrelevant information in the stimulus. This suggests that the processing of endogenous task cues, as opposed to (visually similar) exogenous task cues, impaired participants' ability to suppress interference from a competing task, whether the task switched or repeated. This was the case despite the absence of the expected increase in switch cost for bivalent stimuli.

Implications for theories of control and for working memory

It therefore appears that there were two types of effects of endogenous as opposed to exogenous task cueing. The first category were influenced by cue-target interval. Endogenous task cues affected the endogenous component of task switching, i.e. increased switch cost depending on CTI. Endogenous task cueing also impaired the control of task-irrelevant information in a way dependent on CTI. Neither of these effects was accentuated by additional high CE load. The second category were not affected by cue-target interval, but were accentuated by a secondary task CE load. To summarise, in Experiment 3 there was an overall increase in the effect of task-irrelevant interference regardless of task switching or repetition, and the expected increase in switch cost was absent. There was also a decrease in the switch cost on bivalent trials with high CE load, which appeared to result from a disproportionate failure to control interference where the task repeated. The implications of these two types of effects will be considered separately.

'Specific' cueing effects

The first category can be seen as specific effects of endogenous cueing. In both

cases, performance was impaired by the use of endogenous cueing when, at short CTI, participants were required to process the endogenous cue at the same time as carrying out another function. The two functions that were affected were preparation for a task switch or the control of irrelevant information from a current stimulus. The former has already been discussed, but what are the implications of the latter for accounts of task control and switching? Clearly, the absence of an interaction between the two specific effects suggests that the same mechanism is not involved in preparation for a switch and in the suppression of task-irrelevant information in task switching. This supports the findings of Experiment 2. However, it appears that the control of interference *did* rely on a process also involved in the process of task cueing, although this was not involved in task switching. This is consistent with cognitive models that claim that both the endogenous direction of behaviour, and the suppression of conflicting tendencies triggered by currently irrelevant information from the environment, require executive control (e.g. Norman and Shallice 1980, see Chapter 1).

The findings of the studies so far reported here also place some constraints on such models. Firstly, the 'endogenous' control of behaviour is clearly not carried out by a unitary process, since the control of interference and the preparation for a task switch were found to be unrelated, although the use of endogenous task cues independently affected both. Rubenstein et al also found an interactive effect of task cueing and interference on RT, but not on task switching. They explained this in terms of their production system model by suggesting that, as well as having effects on two distinct executive mechanisms involved in switching, these factors both affect response selection. In the case of strooplike interference, the case for this is well known (see MacLeod 1991, for review), but they also suggest that the production rules involved in response selection may be applied more quickly with explicit task cues (Rubenstein, Meyer et al. In press). These issues warrant further experimental investigation, but this is beyond the scope of this thesis. Whatever the component processes in 'endogenous control', this study established that neither of the two 'specific' effects of the task cueing manipulation discussed here involve the phonological loop or the central executive of working memory.

'General' cueing effects

The second category of differences between Experiments 2 and 3 may be described as 'general' effects of endogenous cueing, in the sense that they do not occur at the time of processing the cue, but affect performance overall. Again, it is possible that the

effects are separate, and mediated by different processes. However, it is worth considering whether they could be causally linked, in order to generate a more parsimonious explanation for the present findings. In discussion of the effects of central executive load on task switching, it was suggested that task control may not operate only when the task switches. So far, control has been assumed to take place exclusively on task switch trials, with repetition of the same task essentially being 'automatic'. If this were the whole story, there is no reason why high CE load should cause slowing of RT on task repeat trials. This slowing was most marked on incongruent trials, where correct responses meant that the participant had not responded using the wrong task rule. Therefore even if one assumes some form of 'task uncertainty' that meant participants were taking extra time on all trials with ambigous (bivalent) stimuli, once this uncertainty was resolved, performing the same task should still have been faster than performing the other task, i.e. switching.

It is possible that control processes could affect switch costs by improving or maintaining improved peformance on task repeat trials, rather than by slowing performance on switch trials. Perhaps the effect of stimulus valence on switch costs with exogenous task cueing depends on active suppression of interference from the other task when the task repeats, rather than on an effect of interference on a switch-specific process. If this were the case, and such control of interference required the central executive, the effect of CE load on switch costs might be explainable. This hypothesis links the effects of endogenous cueing on baseline performance to the effects of the CE on task switching and interference with endogenous cueing, and suggests that endogenous cueing does place a greater demand on the central executive, in the 'general' sense described above. What might be the mechanism of such an effect?

There are precedents in the literature for a role of working memory in the control of interference. The first is Hasher and Zacks' account of cognitive aging, which attributes declining working memory performance to an increase in currently active goal-irrelevant information (Hasher, Rypma et al. 1989). This account does not specifically address the role of the central executive, as it is based on the unitary view of working memory as manipulation plus storage of information (see e.g. Kyllonen and Christal 1990). However, links have been made between this and the central executive in the modular view of working memory (e.g. Gilhooly, Logie et al. 1993). There is more direct evidence of a role for working memory in the suppression of irrelevant

information from other studies. A recent study demonstrated an impairment of negative priming which increased as digit load increased, suggesting a possible CE effect (Engle, Conway et al. 1995). Also, in a series of experiments using the antisaccade task, evidence of poor inhibition of reflexive eye movements was found with a high concurrent memory load (Roberts, Hager et al. 1994). Although it would be unwise to assume that these different forms of inhibition necessarily share common processing, these findings suggest that it may be fruitful to explore an inhibitory role of the central executive of working memory. In the current context, some support is provided for the idea that the use of endogenous task cueing did increase the demand on the central executive, but that this was not a specific effect relating to processing of the cues themselves, but a more general effect relating to the maintenance of task-relevant information in working memory, and the suppression of task-irrelevant information. This hypothesis will be considered directly again in Chapter 4, and issues of inhibitory control will be discussed further in the context of the study of task switching in patients with frontal lobe damage, in Chapter 5.

Conclusions

It is clear that models of task switching cannot address the more general issues of task control raised above. But the suggestion has been made that poor general control of interference may influence switching indirectly, by affecting performance when the task repeats, because of a failure of inhibition of competing task sets. The aim of the next experiment was to address these issues by exploring performance on task repeat trials in more detail, and specifically the role of task set inhibition and inertia in accounting for task switch and task repeat performance.

Chapter 3

Experiment 4: Exploration of progressive effects of task repetition

Introduction

The experiments described in the previous chapter considered specific aspects of task switching performance, and explored the role of the central executive in these by combining switching with secondary tasks. The prediction, based on models of task switching, was that executive control would play a role in preparation for a switch. No evidence was found to support this. Instead, the results indicated that load of the central executive was affecting performance more on task repeat trials than on task switch trials, so that the the time cost of task switching was reduced, rather than increased. This raised the question of how executive control might influence task repetition. The present study set out to examine performance on task repeat trials in more detail. In particular it tested the hypothesis that after a switch, performance improves over several repetitions of the same task, rather than only on the transition from the switch to the first repetition. In order to interpret the findings in the context of models of task switching, attempts were made in particular to distinguish the influence of proactive control processes on task repetition from carry-over effects, such as task set inertia.

Potential sources of task repetition effects

The difference measured as the 'switch cost' can potentially be affected by factors that speed performance when the task remains the same, or factors that slow performance when the task changes. There are two broad ways in which switch costs might be influenced by factors affecting task repetition trial performance. The first possibility is that performance of a new task simply improves gradually with practice, as the task set builds up, or is set up, over a time scale of a individual trials. The second is that there is a gradual improvement in performance of a task with successive trials, but that this is the result of the wearing off or disengagement of carry-over effects from performance of the previous task. The latter formed the basis of the task set inertia

hypothesis of Allport et al (1994). The former possibility, that any improvement is due to proactive processing, can be broken down further into an effect on either of what have hitherto been referred to as the endogenous and the exogenous contributions to the switch cost. The patterns of performance that might be observed under these different circumstances are illustrated in schematic form in **Figure 3.1.** Since the previous two studies have shown that the 'endogenous' control of switching is not synonymous with preparation for a switch, the terminology used by Rogers and Monsell (1995) will not be used further. Instead, the part of the switch cost abolished by preparation will be referred to as the preparatory switch cost, and the underlying processes considered in the more general context of task-specific preparatory processing. In a similar fashion, the part of the switch cost resistant to preparation but sensitive to task-irrelevant interference, introduced by manipulations of stimulus valence, will be referred to by these characteristics. The question of 'endogenous' task cueing, by the words 'switch' and 'stay', will be considered further in the next Chapter.

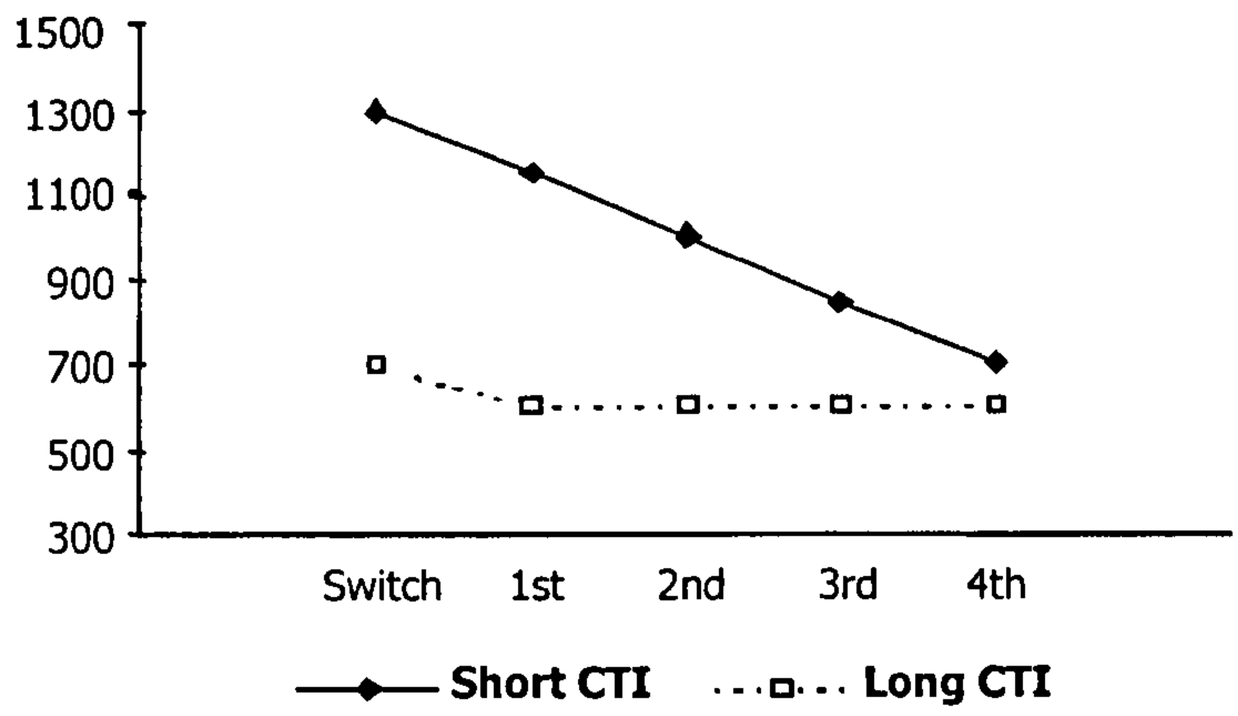

Figure 3.1a. Shows preparatory task repetition effect, with progressive RT speedup only at short CTI. The y-axis represents RT, and the x-axis the progression from a task switch through successive task repetitions. Between the switch and the first task repeat trial at long CTI is shown a residual switch cost.

If performance were found to improve gradually after a task switch, but only when the cue-target interval was short, this would suggest an effect on task-specific preparation. Such a finding would imply that previous repetition of a task only improves performance in the absence of time to prepare for that task, and that this effect is cumulative over several repetitions (**Figure 3.1a**). If, however, there was a progressive RT speedup that was not influenced by preparation, this would imply that previous repetition of a particular task improves performance irrespective of task preknowledge. This would be consistent with a different underlying mechanism, resistant to the effects of preparation (**Figure 3.1b**). **Figure 3.1c** represents the

findings and model of Rogers and Monsell (1995; see below).

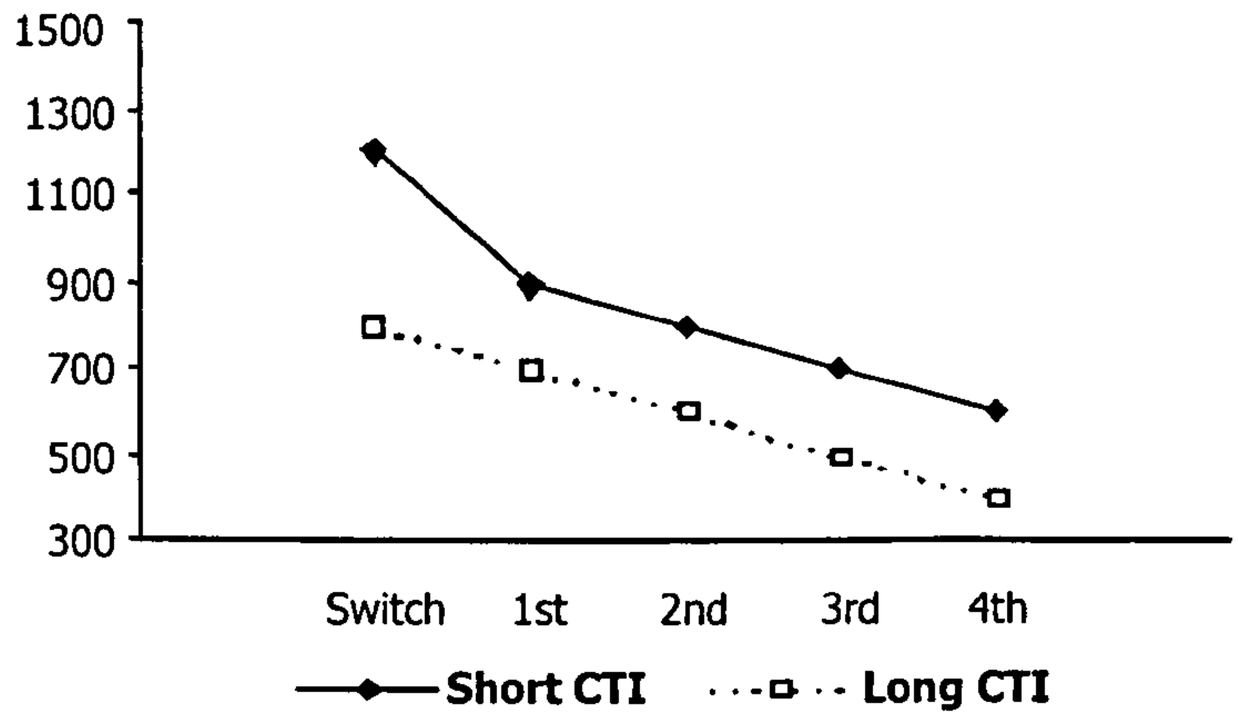

Figure 3.1b. Shows preparation-insensitive task repetition effect, as found by Brown and Marsden (1988). The RT benefit of task repetition is the same at long and short cue intervals. Axes are the same as above. Between the switch and first repeat trial, some extra RT slowing responsive to preparation is shown, consistent with the literature.

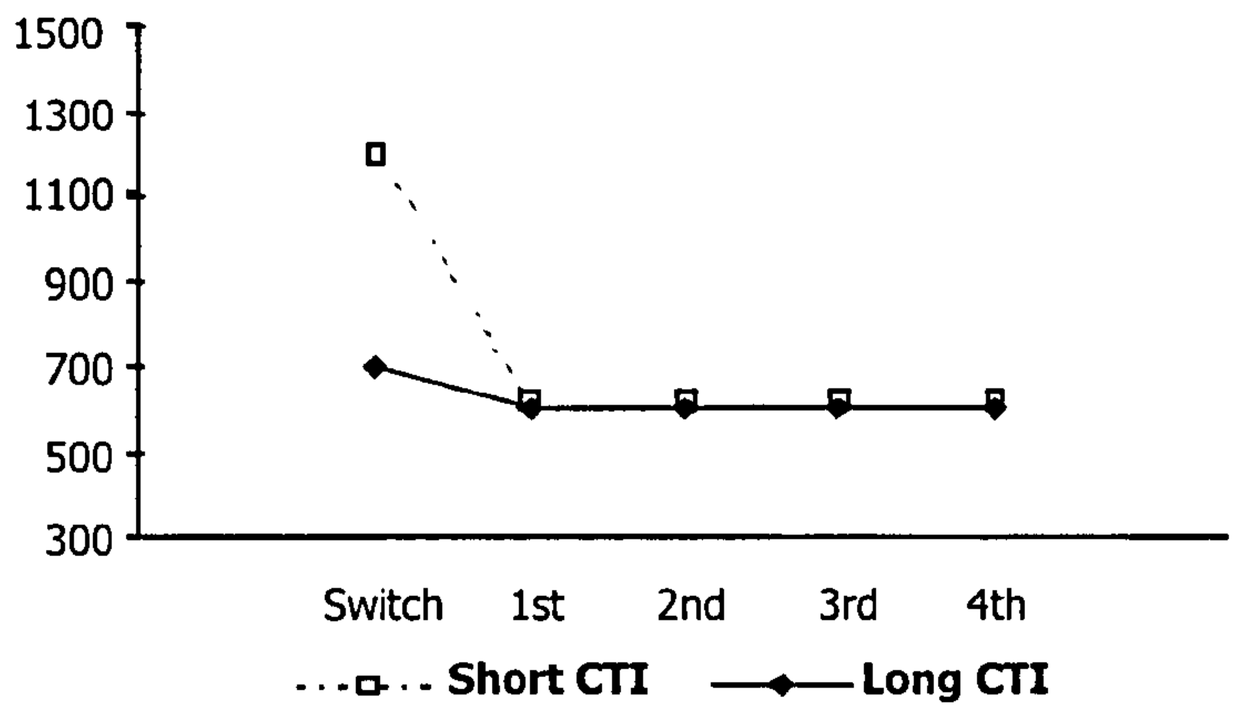

Figure 3.1c. Shows stimulus-cued completion, as found by Rogers and Monsell (1995). There are no progressive RT benefits of task repetition at either long or short CTI, and task repeat RT is the same at both cue intervals Axes are the same as above.

Previous studies of task repetition

Task set inertia

A number of previous studies have examined task repetition performance, in different ways and from various theoretical viewpoints. It has important implications for models of task switching, and in particular for Allport et al's task set inertia hypothesis, and for Rogers and Monsell's postponement account of switching (Allport, Styles et al. 1994;

Rogers and Monsell 1995). Allport et al measured the time cost of alternating between competing strooplike tasks over a series of lists of 7 stimuli, and found that this cost decreased gradually over about 12 lists (Expt. 3). In fact, there were persistent time costs of task alternation only when different tasks had recently been performed with the same types of stimuli (Expt. 4). When discrete trial performance was examined, switch costs were not reliably reduced by preparation (Expt. 5). A further two studies measured the effects of a semantic criterion shift on response accuracy, where word targets were presented in rapid visual sequence (rapid serial visual presentation or RSVP paradigm; Expts. 6 and 7). The latter experiments showed a gradual wearing off of the switch cost over a number of task repetition trials. On the basis of this evidence the authors suggested the time cost of switching was due to a 'task set inertia' (TSI), which resulted from previous processing, and persisted over 'a time scale of minutes'. As mentioned in Chapter 1, this theory has been revised since the present experiment was conducted (see Allport and Wylie 1999; Allport and Wylie In press). The implications of this will be considered when the findings are discussed.

Evidence against task repetition effects

Rogers and Monsell (1995) carried out an experiment aimed specifically at exploring progressive RT effects of task repetition over small numbers of trials (Expt. 6). Their predictable switching studies had already shown that preparation for a switch was possible, suggesting that Allport et al's task set inertia hypothesis could not account for the entire switch cost in all circumstances. The experiment in question was carried out to see whether there was evidence to support TSI as an explanation for the 'residual' switch cost that remained after preparation. If Allport et al's (1994) hypothesis was correct, the residual cost was predicted gradually to disappear over a number of trials. However, TSI would not have been the only possible explanation of such a finding. Another alternative was 'micro-practice'. This term was used to refer to earlier findings that repeated exercise of the same stimulus-response mapping facilitates performance (Rabbitt and Vyas 1973; Duncan 1975), but was also described as "dynamic adjustment... (of) ...task-specific categorization and decision criteria" from trial to trial, optimising performance on the basis of feedback (Rogers and Monsell 1995). In other words, it appeared to include a number of potential processes, some strategic, that could explain task repetition effects, but did not include task set inertia. In this study, however, 'micro-practice' is used specifically to refer to proactive effects that are distinct from preparatory processing and TSI, and that can lead to improvements in

performance of the same task on a time scale of a few trials.

In Rogers and Monsell's experiment, participants switched between 2 tasks in alternating runs of 4 trials. The response-stimulus interval (RSI) was relatively long at 450ms, which allowed enough time for task-specific preparation (Rogers and Monsell 1995). RT was slower on switch trials, as expected, but there was no progressive improvement over three successive task repetition trials. The authors therefore dismissed both TSI and micro-practice as explanations for the residual switch cost. More recently Monsell et al have replicated and extended this finding with predictable alternating runs of 2, 4, or 8 trials in each task (Monsell, Azuma et al. 1998). This pattern of findings was the basis for the claim that 'stimulus-cued completion' can explain residual switch costs. Rogers and Monsell proposed that there is some process involved in performing a new task that cannot be initiated voluntarily or in advance, but must wait until it is triggered by arrival of the stimulus. In addition, there was no evidence in Monsell et al's study of any progressive RT benefits attributable to an preparatory mechanism, since the same pattern of performance was found at short as at long RSIs (between 100ms and 1100ms).

Evidence for task repetition effects

Incidental results from other studies of task switching, as well as those of Allport et al's original (1994) study, have thrown the generality of this finding into question. Meiran et al analysed successive task repetitions in a study where switching was unpredictable, and 33% likely on each trial (Meiran, Chorev et al. In press). This demonstrated a progressive RT speedup, with a reliable decreasing linear trend in RT, over the first 9 trials on which the task was performed. In this experiment there was little time to prepare, as the cue-target interval was short at 117ms. Salthouse et al have found benefits of task repetition on the 2nd as well as on the 1st post switch trial. In this case there was no opportunity to prepare for the next trial as the RSI was 0ms. A switch occurred every 9 trials, but participants were not informed of this, and apparently were not aware of the regularity (Salthouse, Fristoe et al. 1998).

In a neuropsychological study of task switching and repetition in Parkinson's disease patients, Brown and Marsden found evidence in their healthy controls of progressive RT benefits of task repetition (Brown and Marsden 1988). Participants performed alternating runs of 10 trials of colour naming or word reading, either with Stroop or with neutral stimuli. With Stroop stimuli there was a progressive RT speedup from the

1st (switch) trial to the 2nd-4th repetitions, but also from there to the 5th-7th and again to the 8th-10th repetitions. Reliable linear trends confirmed this. No such effect was found for the neutral condition. The cue-target interval was long at 1000ms, although the main aim of the study was to manipulate task cueing, using an endogenous or an exogenous method like that adopted in the previous Chapter. Task cueing had no influence on the effects of task repetition.

Los has also found evidence that two repetitions of the same stimulus type, intact or visually degraded, led to faster performance than a single repetition (or a switch). However, it is debatable to what extent the same factors affect switching and mixing costs under these circumstances as under circumstances where manipulations affecting later processing (such as switching task *rules)* are involved (see Los 1996).

Implications of the evidence

There is clearly conflicting evidence in the literature about whether a switch is completed 'at once', or whether benefits in terms of response times accumulate over several repetitions of the same task. The findings of Monsell and co-workers appear to indicate that a switch *can* be completed in a single trial, whatever the preparatory interval. In their experiments, task alternations were entirely predictable despite varying lengths of runs of trials, up to 8. In these studies, where no progressive repetition effects were found, the response-stimulus interval was varied, but in Meiran et al's (in press) and in Salthouse et al's (1998) experiments, it was short. It is therefore not clear to what extent task uncertainty, and inter-trial or cue-target interval, may influence whether progressive benefits of task repetition are found. However, Brown and Marsden's (1988) study also used predictable task sequences and a long RSI, and repetition effects *were* found. It thus appears likely that some kind of RT savings occur because of effects on an preparation-independent process such as micro-practice or TSI. The absence of such an effect in Monsell et al's experiments could be explained if it were offset by strategically based RT slowing in advance of a switch, if participants were able to begin preparing for the new task ahead of the actual switch trial. This could as easily take place at long as at short RSIs, as no new information is gained during this interval about the task on the next trial. Spector and Biederman (1976) also demonstrated that participants could overlap performance of one trial with preparation for the next, different task, trial.

Preparation and task repetition

Whether a task switch takes place 'at once' has important implications for theories of task switching. An influence of preparation on progressive task repetition effects would be consistent with any model with a process that can activate task sets to a greater or lesser extent before the stimulus arrives, possibly under strategic control, such as Rogers & Monsell's or Meiran's (Rogers and Monsell 1995; Meiran 1999). However, in the latter case, such an account would be at odds with the authors' explanation of their task repetition effect (see below). A progressive task repetition effect found only or predominantly at short CTI would be difficult to reconcile with Rubenstein et al's model, where the preparatory mechanism (goal shifting) appears to be all-or-none and to take place specifically on a switch trial (Rubenstein, Meyer et al. In press).

Salthouse et al's experiments were concerned with the relationship of switch costs and ageing, rather than with component processes (Salthouse, Fristoe et al. 1998). Cognitive ability and age were found to correlate with baseline RT, but not with the switch cost, once this had been corrected for baseline speed. It was suggested that 'higher level processes' were therefore unlikely to have contributed to the cost of switching. This was despite the zero inter-trial interval, which meant that the preparatory component must have contributed to the switch cost.

Other processes in task repetition

As explained above, a micropractice or a task set inertia account of residual switch costs have not entirely been ruled out, so Rogers and Monsell's (1995, and Monsell, Azuma et al. 1998) postponement model remains open to question. Rubenstein et al's (in press) production system model implies that rule activation, triggered by the imperative task stimulus, is all-or-none in nature. If this is the case, the implications of this pattern of results are the same as for Rogers and Monsell's account.

Meiran et al interpreted their finding of a progressive task repetition benefit in the light of their 3 component model of task switching (Meiran 1999; Meiran, Chorev et al. In press), in which the residual switch cost results from 'retroactive adjustment' of the response-set (see Chapter 1 for more details). Assessment of the somewhat confusing claim that participants potentially can prepare the response set, too, is beyond the scope of this thesis and this will not be considered further. The authors claim that

response set, which effectively is an influence of the previous trial task, may be reduced gradually over a number of trials. Despite the fact that the term 'micropractice' was used, this suggestion implies a carry-over effect, albeit of a different kind from that proposed by Allport et al (1994). Other studies by the same group have found evidence that switch costs may dissipate over time since the last trial (Meiran, Chorev et al. In press, Expts. 1 – 3), a characteristic apparently at odds with Allport et al's finding that the cost of a switch was reduced depending on how many subsequent stimuli had been presented, not on elapsed time (Allport, Styles et al. 1994, Expt. 7).

Aims and predictions of this study

Given these considerations, an attempt was made to examine task repetition performance by post hoc analysis of the data from the earlier studies. In Experiment 1, RT was faster on second or subsequent than on first task repetition trials, but only with a short CTI. However, in Experiment 2, although there was some evidence of RT speedup with task repetition, this was not dependent upon CTI. In Experiment 3, the effect of task repetition was not reliable, but this could have been due to the high error rates. Clearly, these findings are not consistent, and there was not enough statistical power in these studies to examine further task repetitions, there seemed to be support here for further exploration of such an effect in the present paradigm.

Two new switching subtasks were introduced for this experiment, involving the semantic classification of pictures and words. This was firstly because, as a patient study was planned, it was felt that participants with brain damage would find less abstract classifications more 'user friendly' and easier to learn. The second point was that the new subtasks enabled the use of stimulus sets with greater than two members. In the studies reported in the previous chapter, there was considerable variation in individual patterns of performance, and it was thought that some of this may have been due to the use of 'nonswitching' strategies, based on the identification of individual stimuli as one (more complex) task, such as "circle or horizontal line, left button". Although larger stimulus sets would not prevent this, it was thought that they might discourage it. Finally, it was felt that findings based on the performance of more than one different task combination would be more solid.

Task repetition and preparation

The main empirical question addressed by this study was whether a progressive RT reduction over several successive task repetitions would be demonstrated, and if so, to what extent this would be affected by two factors known to affect switch cost, preparation and task-interference. An unpredictable switching paradigm was used, partly because this is what had been used in earlier studies reported here. However, this method was also preferred on the basis that, although it does not attempt to control participants' expectancies, their expectations of a task switch should not vary systematically according to task switching or repetition. Preparatory interval, in this case the CTI, was manipulated in order to determine what processes might lie behind any RT benefit of successive task repetitions.

Three main possible patterns of performance were predicted (see **Figure 3.1**). A progressive effect of task repetition only where the CTI is short would suggest that preparatory processing at the level of the task set or rule is affected by previous task repetitions, such that previous performance means that preparing to perform the same task again takes less time (**Figure 3.1a**). Progressive improvement of performance with task repetition, unaffected by the cue-target interval, would suggest an effect independent of preparatory task-specific processing (**Figure 3.1b**). An interaction between task repetition and interference from task-irrelevant stimulus attributes would support this, as the effect of stimulus valence on task switch costs has so far been found to be unaffected by preparation (see previous Chapter). The third possibility was no progressive effect of task repetition, supporting the idea of one-trial stimulus-cued completion of a switch (**Figure 3.1c**).

Task repetition and carry-over effects

This experiment investigated various possible carry-over effects on task repetition and switching. These would be of particular theoretical importance if an exogenous task repetition effect were found (see above). Three separate analyses were planned, to explore different possible inertia-like effects of the *previous task* on current task performance.

Influence of task repetition on a subsequent switch

One way of interpreting the TSI hypothesis was that practice of the same task over a

greater number of trials before a switch to a new task would slow the switching process. Depending on three possible types of carry-over effect, there were two possible predictions for this analysis. If 'inertia' from the previous task determined the time taken to switch, but the effect was independent of competition between tasks, this would be reflected in a longer switch trial RT after a longer pre-switch run in the other task. This pattern of findings was also expected if there were carry-over inhibition of the switched-to task because of competition between tasks before the switch. These two mechanisms would be distinguished by the sequential analysis of valence effects (see below). If there were residual activation of the switched-from task, then a longer pre-switch run of trials was predicted to slow RT on a switch trial only if the stimulus was bivalent.

Inhibition and sequential effects of stimulus valence

The second analysis looked at carry-over effects dependent on competition between tasks, by exploring the effects that task-irrelevant stimulus information on the previous trial would have on performance. Two previous studies of task switching have examinined sequential effects of stimulus valence, in order to investigate more directly the possibility that inhibitory mechanisms may affect performance at the level of the task set or rule. The findings have not been clear cut. Perry found that suppression of previous interference affected the first task repetition, while Rogers et al examined only switch trials, and found slight RT slowing with if there had been task-interference on the last trial (Perry 1997; Rogers, Sahakian et al. 1998). This study allowed for replication and extension of these findings. It should be recalled that the TSI hypothesis does not refer to processes that take place only on task switch trials, but suggests that switch costs are the result of control exterted *prior* to a task switch (Allport, Styles et al. 1994).

Different effects of such inhibition on task switch and repeat trials were predicted. On task repeat trials, if inhibition of the competing task set occurred on the previous trial because the stimulus was bivalent, a reduction in interference from the competing set would be expected on the current trial. On task switch trials, however, inhibition of the *now relevant* task set, because of interference on the previous trial, should lead to a slower switch to that task (and therefore slower switch trial RT) regardless of current stimulus valence. A different pattern of findings was predicted if specific inhibitory processes are brought into play on a switch trial. In this case, carry-over effects of

inhibitory control would be found only on the first task repeat trial, and would be expected to follow the pattern described above.

Asymmetric switch costs

The third analysis concerned the presence of asymmetric switch costs between competing subtasks, as predicted by the TSI bypothesis (Allport, Styles et al. 1994). This was a further reason to introduce new subtasks in the present study. Semantic classification of pictures and words was chosen, as there was evidence that with similar tasks using *verbal* responses, picture classification interferes more with word classification than vice versa (Glaser and Dungelhoff 1984). However, responses were again manual, so that the tasks would be comparable with the ones already used. If the expected pattern of task dominance prevailed, switch costs were predicted to be greater when switching to the picture task than to the word task.

Dissipation of switch costs with time?

This study also examined the effects on performance of time since the previous trial, by manipulating response-cue interval (RCI) as well as cue-target interval (CTI). There were two reasons for including this in the same experiment. Firstly, in discussion of the findings of Experiment 2, concern was raised as to whether participants' expectancies, building up during a long RCI, had affected performance on short CTI trials. This was because, in the studies reported in the previous Chapter, a short CTI was always associated with a long response-cue interval, and vice versa. It was therefore thought to be a good idea to disconfound RCI and CTI in the next study by manipulating both independently.

This afforded an opportunity to replicate Meiran et al's finding of a reduction in RT switch cost as the RCI was increased, consistent with a process whose activity decays with time (Meiran, Chorev et al. In press). At the time of carrying out the study, the support for the authors' explanation in terms of passive decay was open to some question, however, as the effect was not then reliable where intervals were varied randomly within blocks of trials. The effect therefore seemed worthy of replication where intervals are varied randomly. Meiran et al's claim would be supported by a reduction in switch trial RT with a long compared to a short response-cue interval (RCI). If a process specific to task switch trials was responsible, RCI would not affect task repeat trials, or would affect switch trials disproportionately. Since TSI appears to

be reduced by elapsed events rather than time, RCI was not expected to influence the effects predicted above (see Allport, Styles et al. 1994, Expt. 7).

Method

Participants

24 undergraduate and graduate students from the University of Bristol, and members of a subject panel, took part in the experiment in exchange for cash or course credits. They were aged between 18 and 39 years.

Task description

As in the studies in the previous Chapter, participants were asked to switch unpredictably between performing two simple speeded response tasks. However, different subtasks were introduced for the present study, involving semantic classification of pictures or of words, for reasons described earlier. In the picture task, participants were required to classify a picture as an animal or a vegetable, by pressing the left or the right button on the response box. In the word task, the classification was of a word as a tool or a part of the human body, by pressing one of the same two response keys. These are all basic level categories, according to Battig and Montague's classification (Battig and Montague 1969). Pictures were selected from the Snodgrass and Vanderwart set, and additional norms obtained from a paper by Morrison et al (Snodgrass and Vanderwart 1980; Morrison, Chappell et al. 1997). Tool words and human body part words were matched as far as possible on category rank, length and frequency (for tool words, means for these measures were 11.7, 5.0, and 2.7; for body part words, they were 16.1, 4.4 and 3.3). Pictures of animals and vegetables were matched on category rank, visual complexity, and familiarity (for the animals, mean values were 21.8, 3.6, and 2.8, respectively; for the vegetables, they were 23.5, 2.8 and 3.1). For full details of stimuli see **Appendix B.** The appropriate task on each trial was again determined by a fully informative precue.

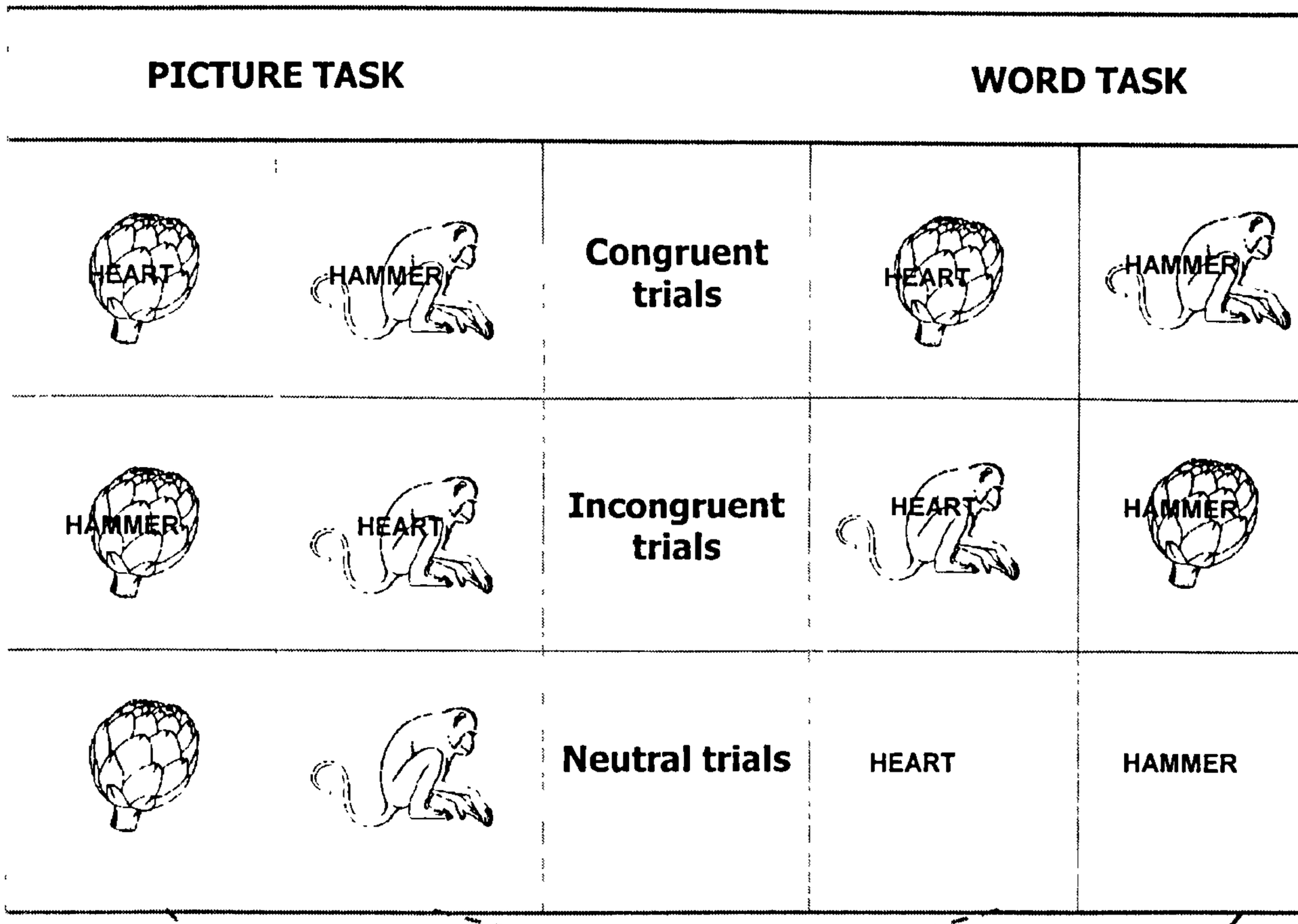

Figure 3.2. Shows the various stimuli and their responses for half the participants. For the picture and the word task respectively, vegetables and parts of the body are mapped to a right keypress, and animals and tools to a left keypress. Captions indicate the stimulus categorisation for that task.

On two thirds of trials, as in Experiments 2 and 3, the stimulus was bivalent, including a to-be-ignored picture or word, and therefore appearing as a word superimposed on a picture. Because the same two response buttons were used for both tasks, this task-irrelevant attribute could be associated with the same (congruent), or the other (incongruent) response. On the remaining third of trials, stimuli consisted only of a picture or a word (univalent trials) (see **Figure 3.2**). A fully informative cue was presented before the stimulus on each trial, to indicate to the participant whether to perform the shape task or the line task

Apparatus and stimuli

The apparatus was the same as that used in Experiments 1 to 3. All target pictures were magnified from the Snodgrass originals, and positioned within an area approx. 5cm by 5cm square on the screen. Target words were displayed in red, in uppercase Arial bold font, size 22, and cue words in black, in size 26 (height of letters on the screen was approximately 7mm). Stimuli again appeared on a panel in the centre of the screen, 6cm tall and 14cm wide. The task cues were positioned laterally within the panel, with their centres approximately 5cm from the fixation point., and were both either the word 'PIC' or the word 'WORD'. The target appeared centrally on the panel. All cues were again fully informative of the task to be performed on that trial.

Design

The experiment manipulated 4 main independent variables in a 5 x 2 x 2 x 3 repeated measures design. These were task switching/ repetition, cue-target interval, response-cue interval, and stimulus valence. Task switching was defined as a sequential effect within blocks, with 5 levels. Task switch trials were those where the participant had performed the other task on the previous trial (picture → word or word → picture), and task repeat trials were those where he/she had performed the same task on the previous trial (picture → picture or word → word). The latter were divided into bins according to the number of successive repetitions of the same task, in such a way as to ensure roughly equal numbers of observations per condition (see Results). The cue-target interval (CTI) and response-cue interval (RCI) were either short and long, and were varied within experimental blocks. Stimuli were either univalent, bivalent-congruent, or bivalent-incongruent. Valence was also manipulated within blocks, using equal numbers of each trial type. The stimulus-response mappings were counterbalanced across participants, for the word task only, to avoid any unpredicted stimulus-response compatibility effects between the mappings for the two tasks (see **Figure 3.2**). The order of introduction of the two subtasks during practice was counterbalanced.

Procedure

The time from the participant's response on the previous trial to the arrival of the cue words for the next trial, response-cue interval (RCI) was either 150ms (short) or

1500ms (long). The target then followed the cue by either a short (150ms) or a long (1500ms) cue-target interval (CTI). The response-stimulus interval thus varied from 300ms to 3000ms. Instructions to participants were identical to those given in Experiment 1. The same response box was also used.

All participants were given 4 blocks of practice, each consisting of 24 trials, to familiarise them with the tasks and procedure. These comprised two blocks of practice with one task alone, followed by two blocks of practice with the other. Cues were presented during practice, although the task was constant, and CTI and RCI were varied as in the experiment.

The experiment consisted of 10 blocks of 49 trials each, of which the first was not recorded, and on which CTI and RCI were always long. The task on each trial was determined randomly, such that there was a 0.25 probability of a task switch and a 0.75 probability of a task repeat. Other trial variables were then randomly selected with equal probability.

Results

Task switching and task repetition

The main aim of this study was to look for progressive reductions in RT on task repetition trials. A subsidiary concern was to see to what extent the task switch trial affected the overall pattern of performance. An analysis was first carried out to establish the pattern of performance over all levels of task switching and repetition. A planned comparison was then made of successive task repeat trials, which did not include the switch trial. Task repeat trials were divided into bins that produced roughly equal numbers of observations per condition (about 10). In this way, the occasional extreme trials from long series of repetitions were excluded. The reason for this was that, although on most trials participants should expect a task repetition (3 times more probable than a task switch), their expectations might become less predictable after many repetitions, increasingly anticipating a task switch. Only runs of task repetitions in which all responses were correct were included.

A repeated measures ANOVA with the following factors was therefore carried out on median RT and error proportions: Task switching/ repetition (switch trials, then 1^{st}/ 2^{nd}/ 3^{rd}- 4^{th}/ 5^{th}- 8^{th} task repetitions), cue-target interval (CTI) (short/ long), response-

cue interval (RCI) (short/ long), and stimulus valence (univalent/ bivalent-congruent/ bivalent-incongruent). In the analysis of RT, there were missing values in 3 cells; these were replaced by condition means. For the comparison of successive task repetition trials, repeated measures ANOVAs were carried out with the factors task repetition (1^{st}/ 2^{nd}/ 3^{rd}- 4^{th}/ 5^{th}- 8^{th} task repetitions), cue-target interval (CTI) (short/ long), response-cue interval (RCI) (short/ long), and stimulus valence (univalent/ bivalent-congruent/ bivalent-incongruent). For this analysis, only effects involving task switching or repetition are reported.

Overall analysis

The effects of preparation and task-interference on task switching and repetition are shown in **Figure 3.3**. Inspection of the data shows that, as in previous studies, response times were slower at the short cue-target interval, and for bivalent stimuli. RT was again slowest on task switch trials. However, there were also clear progressive RT benefits of task repetition at short CTI, up to the 5^{th} to 8^{th} task repetitions. Similarly, there was a progressive speedup of RT from the first task repetition for bivalent stimuli, at both the long and the short cue intervals. For errors, there did not appear to be a progressive reduction in the error rate after the first task repeat trial. Instead, there were a greater number of errors on incongruent task switch trials, at both cue intervals.

For RT, analysis of variance showed reliable main effects of task switching/ repetition ($F(4,92) = 61.83$, $p < 0.001$), cue-target interval ($F(1,23) = 48.12$, $p < 0.001$), and stimulus valence ($F(2,46) = 66.52$, $p < 0.001$). There were also significant 2-way interactions of task switching/ repetition with both CTI ($F(4,92) = 43.09$, $p < 0.001$) and with valence ($F(8,184) = 4.86$, $p < 0.001$), and an interaction of CTI with valence ($F(2,46) = 8.48$, $p < 0.001$). There were no reliable higher order interactions involving task switching/ repetition and either CTI or valence (for 3-way interaction, $F < 1$, n.s.; for 3-way interaction of task switching/ repetition with CTI and RCI, $F(4,92) = 1.38$, n.s.; for 3-way interaction of task switching/ repetition with stimulus valence and RCI, $F < 1$, n.s.; for 4-way interaction, $F(8,184) = 1.43$, n.s.).

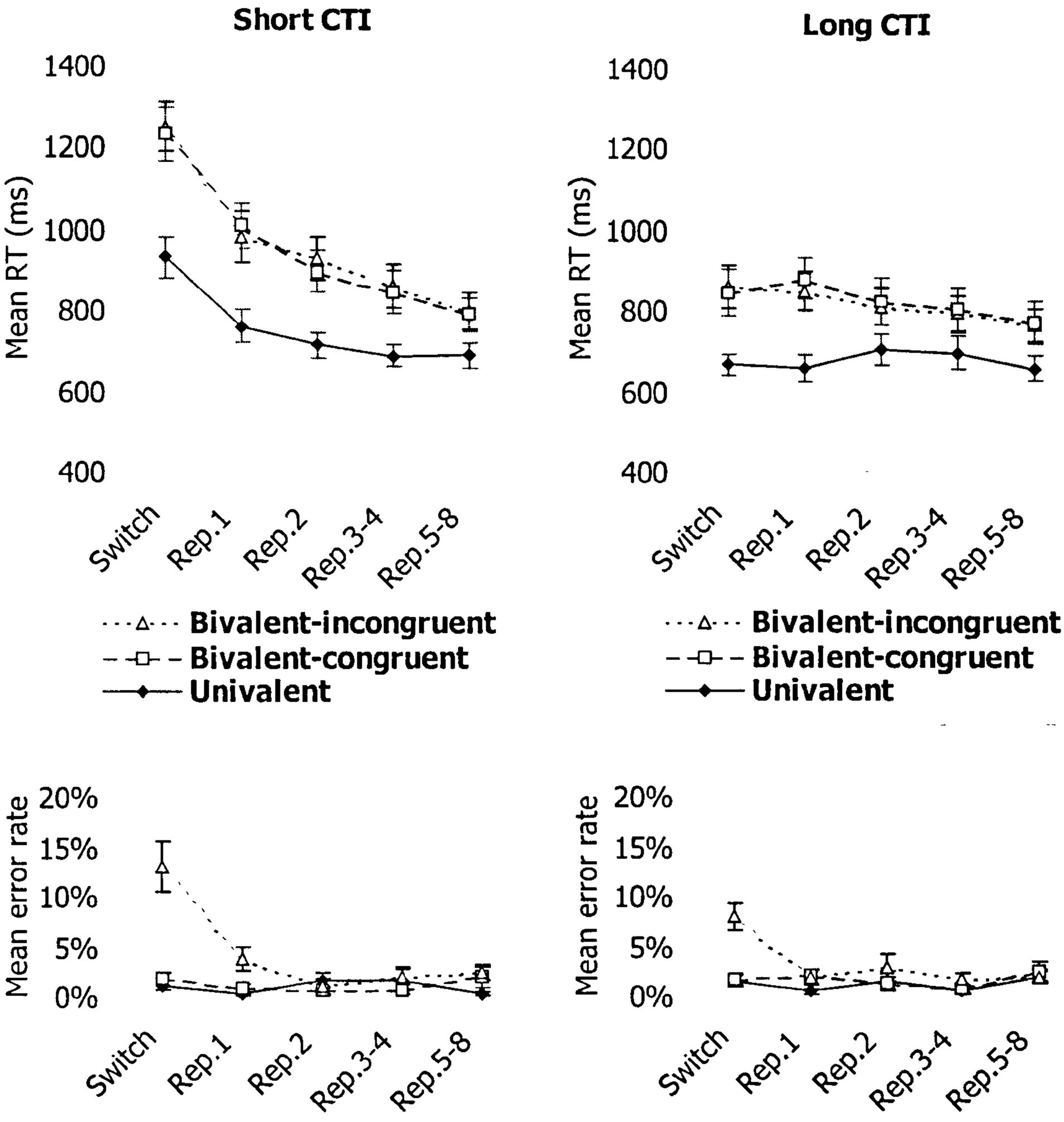

Figure 3.3. Shows the effects of successive task repetitions on RT and errors at short CTI and at long CTI in Experiment 4. Error bars represent the standard error of the mean.

There was some trend towards an interaction of response-cue interval with cue-target interval ($F(1,23) = 3.41$, $0.05 < p < 0.1$). There were no other reliable effects involving RCI (for main effect, $F < 1$, n.s.; for interaction with task switching/ repetition, $F(4,92) = 1.28$, n.s.; for interaction with stimulus valence, $F < 1$, n.s.; for 3-way interaction with CTI and valence, $F < 1$, n.s.).

For errors, there were reliable main effects of task switching/ repetition and valence, and a reliable interaction between these two variables ($F(4,92) = 15.72$, $p < 0.001$; $F(2,46) = 18.79$, $p < 0.001$; $F(8,184) = 12.92$, $p < 0.001$, respectively). There was also a reliable 3-way interaction of CTI, RCI and valence ($F(2,46) = 4.19$, $p < 0.05$). No other effects were significant.

Effect of preparation on task repetition

In the analysis of task repetition trials alone, there was again a reliable interaction for RT of task repetition and CTI ($F(3,69) = 6.18$, $p < 0.005$). As in the overall analysis, the higher order interactions of task repetition and CTI with valence, with RCI and with both of these factors were not reliable.

The interaction of task repetition and CTI was the result of a more marked effect of task repetition at short than at long CTI. The simple main effect of task repetition was reliable for both cue intervals ($F(3,69) = 30.27$, $p < 0.001$; for long CTI, $F(3, 69) = 4.77$, $p < 0.005$). Data from both cue intervals were examined to see if there were reliable decreasing linear trends in RT over successive task repetitions. The aim of this analysis was to establish if the RT reductions were progressive. A significant linear trend was taken to indicate a monotonic decrease in RT, as the scale over which task repetitions were considered was not a linear one. This analysis showed reliable linear trends at both cue-target intervals (for short CTI, $F(1,23) = 52.92$, $p < 0.001$; for long CTI, $F(1,23) = 14.52$, $p < 0.005$). Inspection of **Figure 3.3** confirms that these trends were in the expected direction.

Post hoc comparisons also carried out to determine when performance at short CTI converged with performance at long CTI. These demonstrated reliably different RT on task repeat trials 1 and 2, but not on task repeat trials 3-4 and 5-8 ($T(23) = 4.39$, $p < 0.001$; $T(23) = 3.44$, $p < 0.005$; $T(23) = 1.56$, n.s.; $T(23) = 1.72$, n.s., respectively, with $\alpha = 0.0125$).

For task repetition trials alone, the error data did not reveal any reliable main effect of task repetition, nor an interaction of task repetition with CTI ($F < 1$, n.s.; $F(1,23) = 1.07$, n.s.).

Effect of stimulus valence on task repetition

With switch trials excluded from analysis, the interaction of task repetition and stimulus valence remained reliable for RT ($F(6,138) = 3.83$, $p < 0.005$). The simple main effect of task repetition was significant only for trials with bivalent stimuli (for univalent trials, $F(3,69) = 1.94$, n.s.; for congruent trials, $F(3,69) = 18.76$, $p < 0.001$; for incongruent trials, $F(3,69) = 11.79$, $p < 0.001$). There were reliable linear trends in RT with successive task repetitions for both congruent ($F(1,23) = 82.54$, $p < 0.001$) and incongruent ($F(1,23) = 38.31$, $p < 0.001$) bivalent trials. It can be seen from **Figure**

3.3 that there was a gradual reduction in RT with task repetition. By the 5^{th} – 8^{th} task repetition, however, there was no convergence of RT for bivalent and univalent trials. Post hoc tests confirmed this impression, with RT still being reliably slower on both congruent and incongruent trials than on univalent trials at this point ($F(1,23) = 31.40$, $p < 0.001$; $F(1,23) = 28.62$, $p < 0.001$, respectively; adjusted $\alpha = 0.006$).

For task repetition trials alone, the interaction for errors of task repetition and stimulus valence was no longer reliable ($F(6,138) = 1.36$, n.s.), although there was still a significant main effect of valence ($F(3,69) = 3.98$, $p < 0.05$). There were no significant higher order effects involving task repetition. Pairwise comparisons showed that participants made more errors overall on incongruent than on univalent trials ($F(1,23) = 7.23$, $p < 0.05$; for univalent verus congruent, $F < 1$, n.s.; for congruent versus incongruent, $F(1,23) = 3.89$, $0.05 < p < 0.1$).

Performance on task switch trials

Although the pattern of findings is broadly similar when switch trials were excluded to when they were included, there are some notable differences. It can be seen from **Figure 3.3** that performance on switch trials at short CTI was in line with the trends for task repetition trials. There was a marked reduction in RT from the switch trial to the first task repeat trial, in addition to the speeding of responses with subsequent task repetitions already described. At long CTI, however, switch trial RT clearly did not differ from RT on the first task repetition, even though for bivalent trials there was a subsequent gradual RT speedup with task repetition. This was confirmed by non-significant comparisons between task switch and first task repeat trials, both overall and for univalent trials only ($F < 1$, n.s. for both). There was therefore no residual time cost of task switching, if one constrains the definition of switch cost to be the difference in RT between the switch trial and the *first* task repetition. Taking all levels of task switching and repetition, and the residual cost as the *overall* effect of this variable, there was a residual cost when bivalent trials were included ($F(4,92) = 3.55$, $p < 0.01$), but not for univalent trials only ($F(4,92) = 1.41$, n.s.).

The other difference in the pattern of switch and repeat trial performance was in the error rates. These were increased on incongruent switch trials at both long and short CTI, and unlike for RT, there was no reduction with task repetitions after the first. The interaction of task switching/ repetition with stimulus valence for errors was reliable in the overall analysis, but not when the switch trials were excluded. This error switch

cost was not, however, significantly affected by CTI, as can be seen from the lack of an overall interaction between task switching/ repetition and CTI for errors. The residual error cost of switching, unlike the RT cost, was reliable overall (for comparison of switch and 1st repeat trials at long CTI, $F(1,23) = 14.46$, $p < 0.001$), but as for RT, it was not significant for univalent trials alone ($F(1,23) = 2.51$, n.s.).

Effects of preparation and time since last trial

Preparation (CTI) and time since the last trial (RCI) also affected RT and error rates depending on the presence or absence of task-interference, but irrespective of task switching or repetition. These findings are illustrated in **Table 3.1**, where it can be seen that participants responded more slowly at short than at long CTI, but only when the RCI was long. This finding was unaffected by stimulus valence, and reflected in the trend towards an interaction of RCI and CTI reported above (for simple main effect of CTI at long RCI, $t(23) = 4.86$, $p < 0.001$; at short RCI, $t(23) = 1.76$, n.s.; adjusted $\alpha = 0.025$). Inspection of the data showed that this trend was due mainly to an effect on task repeat trials.

	Univalent		Bivalent-congruent		Bivalent-incongruent	
SHORT CTI	**RT (ms)**	**% Errors**	**RT (ms)**	**% Errors**	**RT (ms)**	**% Errors**
Short RCI	741 (29)	0.2 (0.1)	948 (51)	1.0 (0.4)	950 (55)	4.8 (0.8)
Long RCI	769 (39)	1.7 (0.6)	959 (51)	1.2 (0.5)	974 (52)	4.0 (1.0)
LONG CTI						
Short RCI	696 (36)	0.9 (0.3)	834 (53)	1.1 (0.4)	822 (41)	2.0 (0.5)
Long RCI	656 (27)	1.2 (0.5)	817 (53)	2.0 (0.6)	809 (46)	4.3 (1.0)

Table 3.1. Shows the effects of cue-target interval (CTI) and response-cue interval (RCI) on performance in Experiment 4, according to stimulus valence characteristics. Data have been collapsed over the different levels of task switching and repetition. Mean RT and error data are given, with the standard error of the mean in brackets.

The interaction between CTI and stimulus valence for RT was similar to that reported for the studies in the previous Chapter. It arose because the RT slowing effect for bivalent-incongruent versus univalent trials was reduced at long, compared with short, CTI. The simple interaction effect of CTI and valence was reliable for this comparison ($F(1,23) = 8.56$, $p < 0.01$) but not for congruent versus univalent ($F(1,23) = 4.18$, $0.05 < p < 0.1$), or for incongruent versus congruent ($F(1,23) = 1.06$, n.s.) trials.

In **Table 3.1** it can be seen that the error rate was affected by preparation at short

RCI only, in contrast to the finding for RT. The simple interaction effect of CTI and valence was reliable at short RCI ($F(2,46) = 9.97$, $p < 0.001$), but not at long RCI ($F < 1$, n.s.). At long RCI, the simple main effect of CTI was also not reliable ($F < 1$, n.s.). The interaction at short RCI was due to a difference between performance on incongruent trials and both univalent and bivalent-congruent trials. The simple interaction effect of CTI and valence at short RCI only was reliable for univalent versus incongruent ($F(1,23) = 16.70$, $p < 0.001$), and for congruent versus incongruent ($F(1,23) = 7.68$, $p < 0.05$) trials, but was not significant for univalent versus congruent trials ($F(1,23) = 1.36$, n.s.). At short RCI, preparation reduced only incongruent trial errors reliably (for simple main effect of CTI, $F(1,23) = 10.85$, $p < 0.005$; for neutral trials, $F(1,23) = 3.63$, $0.05 < p < 0.1$; for congruent trials, $F < 1$, n.s.). The 4-way interaction of task switching/ repetition with CTI, RCI and valence for errors was not significant ($F(8,184) = 1.02$, n.s.).

Summary

There was a clear effect of task repetition on RT, with successive repetitions of the same task speeding RT progressively, as predicted. This was more marked at the short than at the long cue-target interval, with RTs converging for the two CTIs at around the 3rd – 4th task repetition. Progressive task repetition benefits were also found only in the presence of task-interference, both congruent and incongruent, but RT for bivalent trials remained significantly slower than RT for univalent trials, even after 5 to 8 successive task repetitions. Response-cue interval did not influence these RT effects. Successive task repetitions had no reliable effects on accuracy. There was therefore evidence of a task repetition effect that was reduced by preparation, and increased independently by the presence of task-irrelevant stimulus attributes. This was consistent with elements of the predicted patterns in **Figures 3.1a**, and **3.1b**, but inconsistent with 'stimulus-cued completion', as in **Figure 3.1c**. The implications will be considered in the Discussion.

When performance on task switch trials was considered, this was clearly affected by preparation, as in previous studies. At short CTI there was a reduction in RT from the switch to the first task repetition trial, but at long CTI there was not. However, this absence of a residual time cost of switching was offset by the presence of a residual error cost, where the stimulus had an incongruent irrelevant attribute.

The time since the last trial, as measured by the RCI, did not reliably affect task

switching in this study, contrary to the prediction made. An unexpected finding was that the effect of preparation on RT was marginally greater when the response-cue interval was long, mainly because of performance on task repeat trials. This difference was not dependent on stimulus valence. Preparation, however, reduced error rates selectively at *short* RCI, and the effect was only reliable for incongruent trials. The meaning of these unexpected findings will be considered in the Discussion.

Investigation of inertia-like effects on switching and repetition

The data were examined for evidence of inertia-like effects of the *previous task* on current task performance. The first question was whether there was any effect of previous task performance on the speed of task switching. Is there some kind of build-up of 'inertia' during previous task performance, that determines the time taken to switch to the other task? If this were the case, then switch trial RT was predicted to be slower, the longer the run of trials just before a switch. Depending on the mechanism, it was expected either to affect all task switch trials, or only bivalent trials. The latter would be found if the effect were due to a build-up of inhibition, because suppression of the to-be-switched-to task occurred when there were bivalent stimuli during the pre-switch run. These effects were examined separately from carry-over effects of previous trial stimulus valence for two reasons. Firstly, it was possible that not all carry-over effects in switching depend upon competition between tasks, and the present analysis was mainly aimed at looking for carry-over activation of the switched-from task. Secondly, there was not enough statistical power here to examine different pre-switch run lengths according also to sequences of stimulus valence. In the present analysis, the effect of interest was on task switch trials only. To maximise the number of observations per condition, the data were collapsed over long and short CTI, and long and short RCI.

Effects of task repetition on a subsequent switch

Task switch trials were therefore divided into bins for analysis depending on how many consecutive pre-switch trials of the other task there had been. These corresponded to those used for task repetition trials. Thus, a switch trial following a switch trial had a pre-switch run length of 1, a switch trial following a one-task-repeat trial a run length of 2, and so on, up to a switch trial following a 5th - 8th task repetition trial, which had a pre-switch run length of 6 to 9. A repeated measures ANOVA with the following

factors was carried out on median RT and error proportions: Pre-switch run length (1/ 2/ 3/ 4-5/ 6-9) and stimulus valence (univalent/ bivalent-congruent/ bivalent-incongruent). Two missing values were replaced by means for the relevant condition (these were both for incongruent trials).

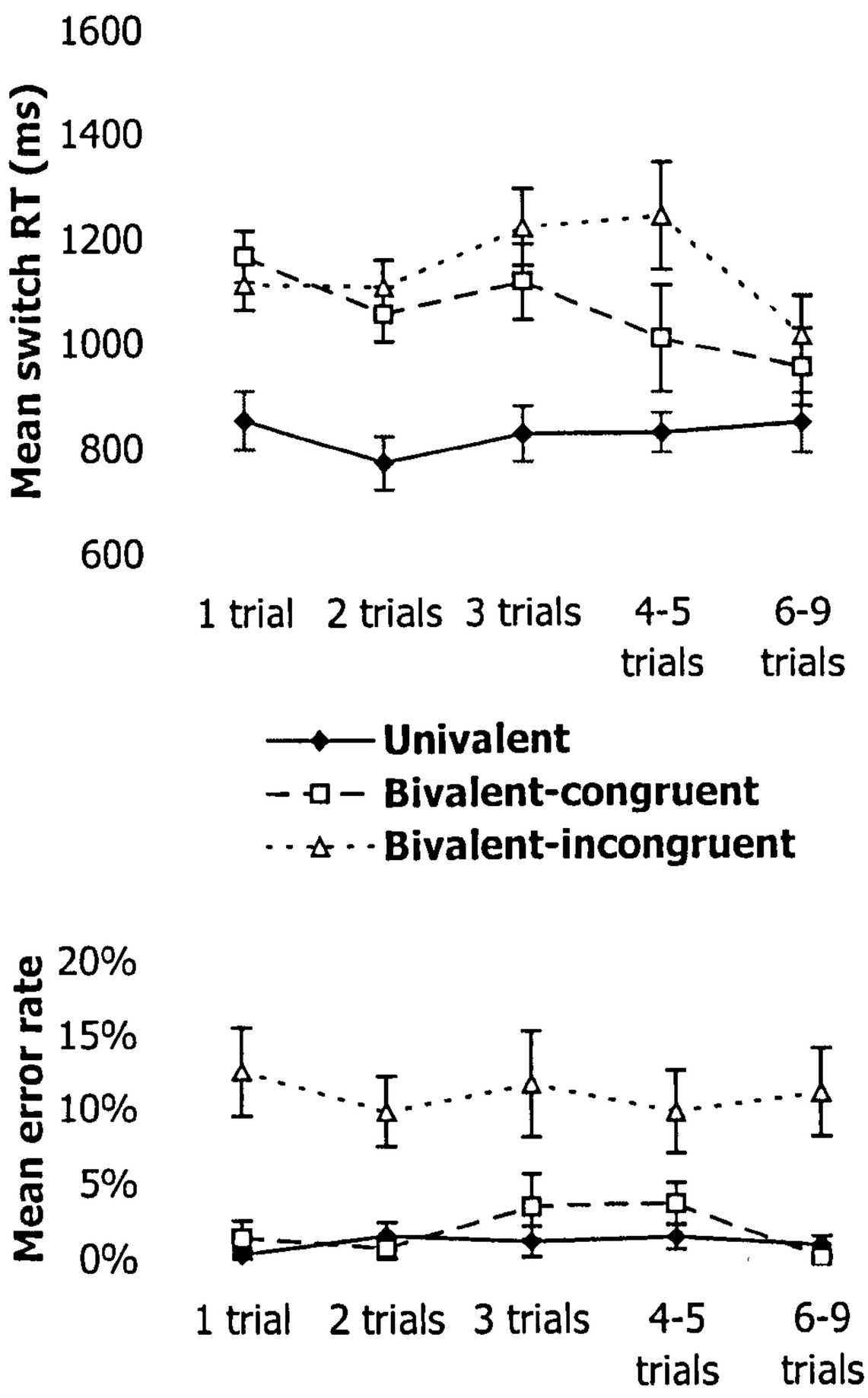

Figure 3.4. The effects of task repetition on a subsequent task switch in Experiment 4. Error bars represent the standard error of the mean.

The mean RTs and error rates for this analysis are shown in **Figure 3.4**. There was a reliable main effect of pre-switch run length ($F(4,92) = 2.91$, $p < 0.05$), and a reliable interaction of run length with stimulus valence ($F(8,184) = 2.49$, $p < 0.05$). Simple main effects of run length were significant for both congruent ($F(4,92) = 4.53$, $p < 0.005$) and incongruent ($F(4,92) = 2.61$, $p < 0.05$) bivalent trials, but not for univalent trials ($F < 1$, n.s.). The data for bivalent trials were then examined for increasing linear trends in RT, and none were found, although there was a reliable *decreasing* linear trend in RT for congruent trials with increasing pre-switch run length ($F(1,23)$

=18.39, $p < 0.001$; for incongruent trials, $F < 1$, n.s.).

Sequential effects of stimulus valence and task set inhibition

The second analysis of intertia-like effects looked at whether there were carry-over effects of the suppression of interfering information, if the stimulus on the previous trial had been bivalent. Unlike the previous analysis, this did not look simply for effects of recent practice of the previous task. Instead, the aim was to determine whether the presence of a task-irrelevant attribute brought an inhibitory mechanism or mechanisms into play that left a measurable carry-over effect on the following trial. Both task switch and task repeat trials were examined. To recap on the predictions, there were two possible patterns of findings here. The first was expected if inhibition of the competing task set or rule took place *on any trial*, and its effects remained on the following trial (task set inertia). In this case, on task repeat trials, if inhibition of the competing task set occurred on a bivalent trial, it was predicted that there would be a reduction in interference from the competing set if the next stimulus was also bivalent. For task switch trials, inhibition of the *now relevant* task set before the switch should lead to a slower switch to that task, and therefore slower overall switch trial RT where the previous trial was bivalent. The second possible pattern of findings was predicted if there were specific inhibitory processes that were brought into play only on a switch trial. In this case, carry-over effects of inhibitory control would be found only on the *first* task repeat trial.

The effects of interest were at the level of the task set, so the important factor was whether task-irrelevant information was present in the stimulus, not whether or not this information was response-congruent. This consideration, and the fact that, in the analysis discussed so far, performance on congruent and incongruent trials had been similar in all important respects, justified the combining of the data from these two trial types. Although the latter was not true of the error data, these were a secondary measure in this analysis and were therefore combined in the same way as the RT data. Response-cue interval was included in the analysis to determine whether any carry-over effects detected dissipated passively over time, but data were again collapsed over long and short CTI.

A repeated measures ANOVA with the following factors was carried out on median RT and on error proportions: Task switching (task switch, then 1^{st}/ 2^{nd}/ 3^{rd} – 4^{th}/ 5^{th} – 8^{th} task repeat), RCI (short/ long), stimulus valence (univalent/ bivalent), stimulus

valence on previous trial (univalent/ bivalent). Trials were only included in the RT analysis if the previous trial was not an error trial or a repeat trial following an error. Three missing values (all on 2nd and subsequent task repetitions) were replaced with condition means. Only effects concerning previous trial stimulus valence will be discussed here. Because of the different predictions, subsidiary analyses were planned to examine the simple main effect of previous valence on switch trials only, and the simple interaction effects of previous and current trial valence on 1st and on subsequent task repeat trials. Error data are only given in detail where they affect the interpretation of the RT data.

The effect of previous trial valence on performance is illustrated separately for different levels of task switching/ repetition in **Figure 3.5.** The major feature of the findings was that on bivalent task repetition trials, RT was faster if there had also been a previous bivalent trial. Analysis showed a reliable main effect of previous trial valence on RT ($F(1,23) = 10.78$, $p < 0.005$). There was also a significant interaction of previous and current trial valence ($F(1,23) = 8.10$, $p < 0.01$). The main effect of RCI was not reliable ($F < 1$, n.s.), but there was some trend towards an interaction of RCI with previous trial valence ($F(1,23) = 3.74$, $0.05 < p < 0.1$). The 3-way interaction of RCI, current trial valence and previous trial valence was not significant, and nor was the 4-way interaction ($F(1,23) = 1.18$, n.s., and $F < 1$, n.s.). There were no reliable findings involving previous trial valence for errors.

On task switch trials, the interaction of previous and current trial valence was not reliable ($F < 1$, n.s.). The predicted simple main effect of previous valence was also not reliable on task switch trials ($F < 1$, n.s.). There was some evidence of an interaction of previous and current valence on 1st task repeat trials ($F(1,23) = 5.87$, $0.017 < p < 0.05$, corrected $\alpha = 0.017$), and a reliable effect on 2nd and subsequent task repeat trials ($F(1,23) = 6.83$, $p < 0.017$). There was therefore no selective effect on 1st task repeat trials. Further follow-up tests were carried out for all task repeat trials together. Analysis of simple main effects of previous trial valence showed that a previous bivalent trial speeded RT if the current trial was bivalent ($F(1,23) = 27.98$, $p < 0.001$), but did not have a reliable effect if it was univalent ($F < 1$, n.s.). The trend towards an interaction of with previous trial valence with RCI arose because participants generally responded faster if the previous trial was bivalent than if it was univalent, but this was mainly due to performance at long RCI, where mean RTs were 803ms and 856ms, respectively. At short RCI, they were 814ms and 818ms. If there

had been a carry-over effect that dissipated with time, a selective effect of events on the previous trial would have been expected where the RCI was *short*. The pattern of findings, was, in fact, in the opposite direction, with an effect mainly at long RCI.

Comparison of picture and word tasks

The third analysis addressing task set inertia compared performance in the picture and the word subtasks. As described above, it was expected that the picture task would interfere more with the word task than vice versa, that is, there would be more RT slowing on word trials from irrelevant pictures than the other way around (Glaser and Dungelhoff 1984). If this pattern of performance was found, it was then predicted, according to Allport, Styles & Hsieh's (1994) task set inertia hypothesis, that switch costs would be greater when switching to the picture task than to the word task.

The first step was therefore to determine whether interference between the two subtasks was asymmetric, before considering differential switch costs. In the picture subtask, mean RTs were 675ms, 876ms and 873ms for univalent, congruent and incongruent trials respectively, whilst in the word subtask, they were 754ms, 907ms and 928ms. The equivalent figures for error rates for the picture subtask were 0.5%, 1.1% and 4.3%, and for the word subtask were 1.6%, 1.4% and 2.7%. There was therefore minimal evidence for a strooplike pattern of asymmetric interference between these two tasks, so detailed analysis of switch costs is not presented. The reason for the higher rate of errors on incongruent picture task trials, in fact, appeared to be different speed-accuracy trade-offs in switching, with switches to the picture subtask being faster and less accurate than switches to the word subtask.

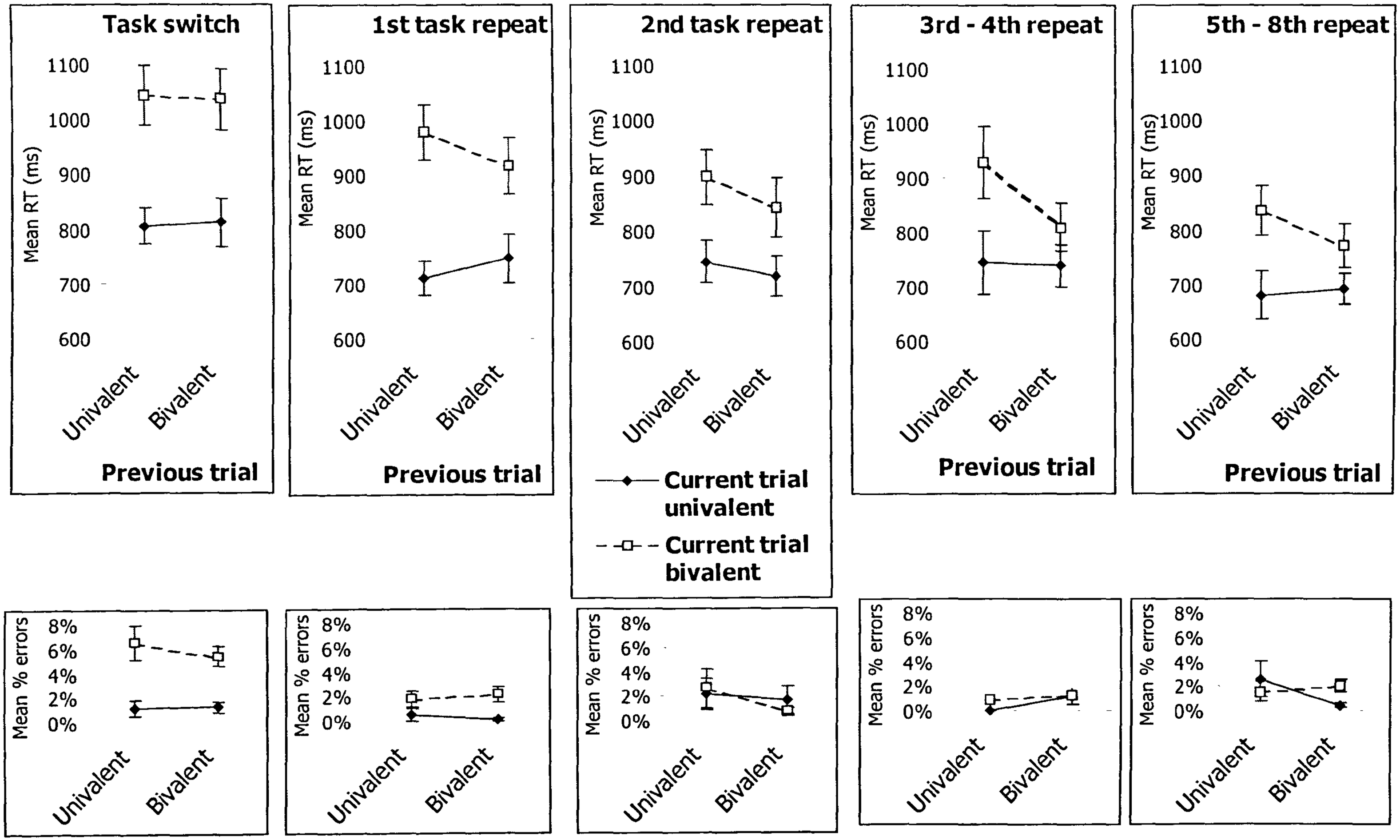

Figure 3.5. Effects of previous trial stimulus valence on performance in Experiment 4. Data are shown according to whether there was cr osstalk on the current trial. Error bars represent the standard error of the mean.

Summary of findings of inertia-like effects

There was clearly no evidence here for the predicted inertia-like increase in switch trial RT with increasingly long pre-switch runs of trials in the other task. The effect in the opposite direction for congruent trials was unexpected, and was not matched by a similar effect on either incongruent or univalent trials. Its implications will be considered in the Discussion.

The presence of interfering information from the alternative task on the previous trial was found to influence performance on the current trial. The effect found fit the predicted pattern for task set inertia for task repetition trials, where interfering information then slowed performance less on the current trial. Contrary to the predictions of TSI, no effect of previous trial valence was found on task switch trials for RT or for errors. However, there was also no evidence of a selective effect on the first trial on which the task repeated, as was expected if a process specific to task switching had been responsible. It is also noted that the carry-over effect on task repeat trials was not found to be affected by the time that had elapsed since the last trial, the response-cue interval.

There was minimal evidence for asymmetry of strooplike interference between the two subtasks, so the predictions of the task set inertia hypothesis in terms of asymmetric switch costs could not meaningfully be assessed.

General Discussion

Cumulative effects of task repetition

The main finding of this experiment was that there was a progressive RT benefit of task repetition over a scale of between one and eight trials. Two possible patterns of task repetition effects had been predicted. The first was a reduction in RT with successive repetitions of the same task, but only at short CTI, suggesting an effect on preparatory task-specific processing (see **Figure 3.1a**). The second entailed a progressive effect of task repetition that would be independent of CTI, suggesting an effect on task-specific processing resistant to preparation (see **Figure 3.1b**). The results of this study provide support for both these predictions. The task repetition effect on RT was more marked at short CTI than at long CTI, with RT at the two cue intervals converging at around the third to fourth task repetition. The task repetition effect was also more pronounced for bivalent stimuli, although RT was still reliably slower than on univalent trials after five to eight task repetitions. The influences of preparation and of stimulus valence on task repetition appeared to be independent, so although preparation reduced the RT benefit of more than one repetition of the same task, it did not abolish it. There was no evidence here for the third possible pattern of performance, stimulus-cued completion of a task switch, which was predicted by Rogers and Monsell's postponement account of switching (see **Figure 3.1c**). The present study therefore supported the notion that a switch to a new task is not an all-or-none event, despite the findings of Monsell and co-workers suggesting that it may be so under certain circumstances (Rogers and Monsell 1995; Monsell, Azuma et al. 1998). The improvement in RT demonstrated here after the first task repetition replicated the findings of other previous studies (Brown and Marsden 1988; Salthouse, Fristoe et al. 1998; Meiran, Chorev et al. In press).

Mechanisms in task repetition and task switching

The factors that were found to influence the progressive task repetition effect demonstrated here were the same factors that have been shown to influence task switching in this and other studies. As the present study makes clear, variables can influence the task switch cost by an effect either on task switch or on task repeat trials. In all the experiments reported in the previous Chapter, switch cost was reliably

influenced by preparation, thus replicating the findings of previous studies (Shaffer 1965; Rogers and Monsell 1995; Meiran 1996). However, both Rogers and Monsell's model of task switching, and Meiran's (1996) 2-component model, relied on the presence of a residual switch cost after preparation to distinguish at least 2 processes in task switching. For present purposes, a residual cost could represent either a difference between RT on the switch and the first task repeat trials, or the total difference in RT between the switch trial and the fastest task repeat trial. If the task set is incompletely configured on the first repeat, the concept of residual cost may become less helpful. This will be discussed further in Chapter 6.

In this study, the first task repetition (or the switch) produced a clear reduction in RT at short CTI, although not at long CTI. There was, however, a reliable overall effect of task switching/ repetition at long CTI for bivalent trials. The error data showed poorer accuracy on task switch trials at long CTI, but only for incongruent trials. There was therefore no evidence of a reliable residual switch cost here, according to either performance measure, on univalent trials. Although the incongruent trials were likely to represent 'failures to switch' (see Chapter 2), interfering stimulus information associated with the other task clearly influenced people's ability to switch at long CTI in some way. These findings show that task switch trial and task repeat trial performance were influenced by the same factors. Whether or not residual switch costs are found in the absence of interference between tasks, the time cost of a task switch is generally increased by such interference, and this effect has not been found to be influenced by preparation (Rogers and Monsell 1995, also see previous Chapter). There is therefore good evidence for at least two processes contributing to switch costs. One enables preparation for the new task, but does not influence the control of interference from bivalent task stimuli. Another part of the switch cost is affected by task-irrelevant information, and may only exist in its presence. It is exogenous in the sense of being linked to external triggers (stimulus attributes) that cause interference from an irrelevant task set or rule. The effects of task repetition on these two types of processes in the present study will first be considered separately, and then their implications assessed for current models of switching and task control.

The effect of CTI on task repetition

This study demonstrated that RT speedup after an initial task switch was more marked at short CTI, suggesting that preparation of a particular task set or rule can improve

performance when the task rule is the same as on the previous trial, as well as when it is different. One progressive effect of task repetition is therefore to reduce the amount of time that the preparatory task control mechanism takes, hence the progressive reduction in the *difference between the RT task repetition effects* for long and short CTI. As only two cue-target intervals were used in the present study, it is not possible to determine whether the *rate* of preparation was affected, or whether the control process had 'less work to do' in some sense. This is consistent with the position, voiced earlier, that more than one control process operates at the level of the task set or rule. The greatest difference between long and short CTI in the RT effect of a task repetition was, however, between the task switch and the first task repeat trial, so the possibility remains that some preparatory processing takes place that is specific to a task switch. However, as the decrease in the effect of preparation for a given task with task repetition was gradual over several trials, there is no evidence here to support this.

The fact that different factors may affect preparation for a response and preparation for a switch has been commented upon previously (see Meiran, Chorev et al. In press). It is therefore important to note here that the effect of CTI on task repetition is, like the effect on RT switch cost, a *difference* in the effect of preparation on RT depending on the sequence of subtasks. Although preparation for a response and the effect of factors such as phasic arousal were not manipulated in this study, there is no reason to expect them to affect differential task repetitions any more than they affected the comparison between task switch and task repeat trials in Meiran et al's studies. Note that, for this reason, the absolute difference in RT between long and short CTI, and to some extent the point of convergence of RT at the two different cue intervals with task repetition, is not of interest in the present experiment. It is therefore the different slopes of the task repetition effects, as established by interactive effects of preparation and repetition with differing linear trends, that is important here.

This finding supports earlier demonstrations of progressive task repetition effects with short preparatory intervals, by Meiran et al (in press) and Salthouse, Fristoe et al (1998). It is discrepant with the recent findings of Monsell et al, did not find reductions in RT after the first task repetition, even with a varying response-stimulus interval (Monsell, Azuma et al. 1998). The finding here of an preparatory component to the task repetition effect supports the possibility that, with predictable switching, preparatory set could be adjusted in anticipation of an upcoming switch trial. As

mentioned in the Introduction to this chapter, this is consistent with findings of Spector and Biederman (1976) regarding stimulus preview.

To what extent can different cognitive models account for this progressive task repetition effect dependent on CTI? As suggested in the Introduction to this chapter, both Meiran's and Rogers and Monsell's models could potentially include graded levels of task set activation and/ or variable rates of activation by an endogenous mechanism (Rogers and Monsell 1995; Meiran 1996; Meiran 1999). However, neither is well enough specified at present to make any particular predictions about how to explore this further. Meiran's most recent series of studies has specifically investigated the possibility that preparation for a switch is preparation of a stimulus set, in the sense of attending to different relevant stimulus dimensions (such as, in this case, pictures or words) (Meiran 1999). However, as pointed out in Chapter 1, although the preparatory component of switching was reduced when non-overlapping stimulus sets were used for the two tasks, i.e. when all stimuli were univalent, there was still some effect of preparation on a switch. The model is also insufficiently constrained in terms of what can be prepared.

Of Rubenstein et al's two control processes in switching, the first, goal shifting, is flexible in terms of timing, and can take place before arrival of a stimulus (Rubenstein, Meyer et al. In press). It is therefore endogenous, in the sense used by Rogers and Monsell (1995) and by Meiran (1996). However, the authors also imply that goal shifting takes place specifically on a switch trial, and is all-or-none in nature. In order to account for the preparation-sensitive task repetition effect in terms of this model, one would have to assume that goal shifting can be incomplete. This will be considered further in Chapter 6.

Although Salthouse et al found benefits of at least two successive task repetitions at zero RSI, they suggested that the lack of a correlation of switch cost with general ability and ageing implied that 'higher functions' were not involved (Salthouse, Fristoe et al. 1998). This vague claim is difficult to assess in the present context, because the theoretical frames of reference are different. However, it is worth noting that the results of Experiments 2 and 3 suggested (if anything) that broader cognitive control was involved in the effects of stimulus valence on switching, not in the 'endogenous' component of a switch, that is preparation. The present findings may, therefore, be more consistent with those of Salthouse et al than they first appear, as his switching/

repetition effect was probably mainly due to the preparatory process.

Effects of interference on task repetition

The foregoing discussion has considered the preparatory component to the task repetition benefit found in the present study. However, stimulus valence was found to affect task repetition at both cue intervals, suggesting an independent effect of preparation and task-irrelevant information on the tendency for RT to decrease progressively with repetition of the same task. This suggests the *additional* involvement of a process that produces benefits of task repetition regardless of how much preparation takes place.

This finding is consistent with data from the healthy controls in Brown and Marsden's (1988) study, described earlier. It is not consistent with Rogers and Monsell's account of 'stimulus-cued completion' of a task switch, because this predicts a difference in RT only between a switch trial and a first task repeat trial. Although developed to explain residual switch costs, the hypothesis is in a sense independent from this issue, since it is increasingly clear that residual costs are not always found (e.g. see Meiran 1999). This will be considered more in Chapter 6. The idea of stimulus-cued completion relies more fundamentally on a task switch always being complete 'at once'. It may be that this is the case under certain conditions. However, the finding in this study of an effect of task repetition apparently independent of task preknowledge is difficult to explain in terms of uncertainty about the task to be performed. This notion would predict progressive reductions in RT only at short CTI in the present study. Salthouse et al's (1998) result could be explained in this way, as the RSI was 0ms and the participants apparently unaware of the predictability of switching, but Brown and Marsden's (1988) could not, because the RSI was long and switching again predictable, and task repetition effects dependent on stimulus valence. Could Monsell et al's (1998) results be accounted for by a tendency to prepare partially for the switch to a new task on the preceding (task repeat) trials? If this occurred, one would expect some RT slowing coming up to a task switch, which could offset the preparation-independent task repetition benefit. This seems the most likely explanation of the discrepant findings.

Meiran's group attributed their (incidental) finding of a task repetition effect to 'micro-practice', but offered no clear explanation in terms of their model of task switching

(Meiran, Chorev et al. In press). The preparation-independent task repetition effect here was clearly event- and not time-related (see below). The 'dissipating' or 'waiting' process, thought to cause reduction in switch costs with RCI, could not therefore be responsible (see Meiran 1999). It is possible that 'retroactive adjustment of a response task-set', the mechanism invoked to explain residual costs by Meiran et al, could be a gradual process, taking place over a number of trials. However it is difficult to generate predictions from this at present, due to the claim that participants can, in unspecified circumstances, prepare response task-sets as well as stimulus task-sets (Meiran 1999; Meiran In press).

Can Rubenstein et al's model account for this interference-dependent task repetition effect? According to them, irrelevant stimulus attributes affect task switching by acting on the second control mechanism, rule activation, and the latter may not even be involved in the switching process if stimuli are univalent (Rubenstein, Meyer et al. In press). In the same way that a task repetition effect dependent on task cueing suggests that goal shifting may be partial, the model can only account for this exogenous task repetition effect by allowing rule activation to produce degrees of activation and inhibition of the competing task rules.

On the surface, the present results appear consistent with Allport et al's (1994) hypothesis of gradually reducing task set inertia, and this will be considered in more detail below after discussion of the relevant findings from this study.

Inhibition and carry-over effects

Pre-switch run length

The effect of task repetition on the speed of a subsequent task switch was also examined in this experiment. It was predicted that if a task set built up gradually over a few repetitions of the same task, and if this had to be inhibited in order to switch to a new task, that switch trial RT would be slower the longer the pre-switch run of trials. This was one way of conceptualising 'task set inertia', in which the assumption was made that the system can only be 'set' for one task at a time, and that this set can be variable in degree, and becomes stronger each time the task is performed. Performance of a new task was then expected to be slower the longer the pre-switch run of trials, because previous task 'set' would have been stronger, regardless of

stimulus valence. On the other hand, a concept of TSI closer to Allport et al's original (1994) hypothesis was that carry-over effects depend on competition between tasks. These could derive either from residual activation of competing task rules, or from residual inhibition of the to-be-switched-to task. In the former case, the expectation was that pre-switch run length would not affect switching unless the stimulus on the switch trial was bivalent, causing interference from the still active switched-from task rule. From the latter viewpoint, if during the pre-switch run of trials the to-be-switched-to task was inhibited, then switch trial RT should be slower after a longer run regardless of stimulus valence. The influence of interference between tasks on the switched-from trial was considered separately (see below).

The results showed no evidence of the predicted increase in task switch trial RT when the pre-switch task had been performed for longer runs of trials. This was true whether the switch trial was univalent or bivalent. There was therefore no evidence for a build-up, over the scale of 1 to 8 repetitions of a task, of 'inertia' affecting performance on a subsequent switch to another task. This pattern of performance is consistent with the findings of a study by Fimm et al, who looked at the effects of run length on switch trial error rates in patients with Parkinson's disease and healthy controls (Fimm, Bartl et al. 1994). No inertia-like effects were found, and run-length did not affect performance reliably at all in the control group. In the present study, the finding of a trend in the opposite direction on congruent-bivalent trials only is difficult to explain. This is partly because the prediction of effects at the level of the task rule led to the expectation that RT would follow a similar pattern on all bivalent trials. Could a switch back to the original task become *easier* after a greater number of trials have been performed in the intervening task? This would not be consistent with TSI if inertia-like effects on switching derive from competition between tasks prior to the switch. It could, however, be explained if inhibition of the switched-from task occurred only at the time of the switch to the new task. An analysis of the effects of stimulus valence on the previous trial specifically considered these possibilities.

Sequential interference effects and inhibition of a task rule

The sequential effects of stimulus valence in this study were examined to see if there were carry-over effects of competition between the two tasks, such that relative inhibition of a task on the previous trial might affect subsequent performance of that task. The possible predictions were that this would affect either all trials, suggesting a

pervasive process, or only the first task repetition, suggesting a carry-over from events specific to a task switch. Results showed evidence of an effect of previous stimulus valence when the task repeated. Where the stimulus on the previous trial included irrelevant attributes associated with the other task (such as a picture if the task was word classification), then such irrelevant information caused *less* interference on the current trial. The findings are supported by those of Perry, who found evidence of a similar effect on task repetition trials, although he only examined the first task repetition (Perry 1997). There was no hint here, however, that the effect described was specific to the first task repetition. The hypothesis of a switch trial specific inhibitory control process can therefore be rejected in this context.

The TSI hypothesis describes switch costs as resulting from control exerted prior to a switch, and the present findings are consistent with this. The effect found could in principle be the result of facilitation of some aspect of the current task set or rule, as well as of inhibition of the competing set. However, the reliable and selective effect of previous trial valence on current bivalent trials, and not on univalent trials, in this experiment suggests that inhibition is responsible. If the task being repeated was activated more where the previous stimulus was bivalent, then the result should be a faster response on the subsequent trial even if this was univalent.

The other prediction derived from the TSI hypothesis was that a previous bivalent trial would slow RT on task switch trials. The data from this study did not confirm this, so there was no evidence that carry-over effects of prior control affect the switch to a new task, specifically. It is theoretically possible that carry-over effects on task switch trials might not normally be found, be minimal, or be released rapidly in healthy people. In a recent study of task switching in patients with frontal lobe damage and Parkinson's disease, this appeared to be the case (Rogers, Sahakian et al. 1998). Only task switch trials were examined, and there was some evidence of small effects of previous inhibition in their healthy control groups. Their performance was not examined separately from that of the patient groups, so it was not clear which of the differences were reliable, but in general there appeared to be no effect of previous trial valence. Depending on current trial valence, there was between –5ms and 14ms slowing on a switch trial from a previous bivalent trial. However Meiran (in press) has recently found effects of previous valence on both task switch and task repeat trials. With unpredictable switching between two spatial classification tasks, a previous bivalent, compared with univalent, stimulus reduced RT by about 90ms on task repeat

trials, and increased it by about 65ms on task switch trials (but only at short CTI in the latter case).

It is perhaps not very helpful to attempt to attribute the discrepancy to Meiran's use of a spatial classification task without an attempt at replicating the present findings with more task switch trials (outnumbered by task repeat trials by 4 to 1 here). However, a more relevant point may be that in his study the univalent stimuli consituted four distinct locations, different from the bivalent stimuli, so stimulus repetition was confounded with the sequential effect of valence, occurring on 25% of bivalent-bivalent trial pairs but not on univalent-bivalent transistions. Effects from repetition of individual stimuli as well as of the task set may have been involved, a factor that could not have influenced the present findings since no stimuli, either targets or distractors, were repeated on adjacent trials. This point is not purely theoretical, since recent data from Allport and Wylie showed that some negative priming effects (equivalent to a slower switch to a previously ignored or suppressed task) applied only to individual stimuli, not at the level of the task set or rule (Allport and Wylie 1999; Allport and Wylie In press). In any case, this experiment found no evidence that performance on a task switch trial was selectively influenced by inhibitory mechanisms, and some evidence that they played a part in task repeat trial performance.

There is a further possibility that warrants consideration. Could the present findings could be due to a strategic effect, whereby participants might (at least some of the time) have responded to univalent stimuli as if they demanded a separate 'third task'? If this were the case, the relative speedup in responding on bivalent-bivalent transitions compared with univalent-bivalent transitions could have been because the latter were effectively switch trials, not because of carry-over inhibition. This would also account for the absence of sequential valence effects on switch trials, as here both transitions would involve a switch. There are two difficulties with this argument. The first is that, because switch costs were found to be reliable on univalent trials, such a strategy was unlikely to be particularly prevalent. The second is that the overall pattern of effects of preparation in this study suggested that participants were using the cues effectively to prepare for the two intended tasks, so one would expect that a long CTI would at least reduce the likelihood that a participant would use the '3 tasks' strategy on a univalent trial. Exploratory analyses suggested no increase in previous trial valence effects at short CTI, so such a strategic explanation appears unlikely. This is worth mentioning here because it will be raised again in the following Chapter, also

in the context of univalent trial task repetition effects and sequential valence effects. Also, the influence of CE load on these carry-over effects will be examined. Furthermore, it is worth noting that in Meiran's (1999) studies of sequential stimulus valence effects, the switch cost effect was only found at short CTI, consistent with this interpretation.

Other inertia-like effects

The examination of performance on the picture and word subtasks did not find any good evidence of strooplike asymmetric task dominance. It was therefore not possible adequately to test the predictions of the TSI hypothesis with respect to asymmetric switch costs. However, experiments carried out since this study was completed have shown that this phenomenon is, at best, inconsistent (Monsell, Yeung and Azuma in press), and can, in some cases, be due to criterion effects (see Los 1996). Recent work by Allport and Wylie has led to substantial revision of the idea of task set inertia (Allport and Wylie 1999; Allport and Wylie In press). This is discussed in more detail later.

The present study also examined the effect on task switching and repetition of the elapsed time since the last trial. This time-based carry-over effect was not part of the TSI hypothesis, but had been demonstrated by Meiran et al (in press) in three experiments. Task switch cost was found to be diminished with increasing response-cue intervals, in a fashion consistent with exponential decay. The authors attributed this to a process specifically involved in switching, whose effect decays passively with time after a switch. In the present experiment there was no evidence to support a selective effect of RCI on task switch trial RTs. RCI also did not selectively affect RT on task repetition trials. This study was therefore unable to provide support for Meiran et al's hypothesis.

There were, however, some reliable effects of RCI on performance. Preparation reduced incongruent trial errors at short RCI only. As most errors were on switch trials, this could in theory have been due to declining activation of the switched-from task set during the inter-trial interval, leading to a reduction in the frequency of responding according to the currently inappropriate task rule. However, at long RCI the *overall* error rate was similar to that at short RCI, although there was no effect on it of CTI. Meiran et al's account would predict a uniformly low rate of incongruent

switch trial errors at long RCI, or a declining rate depending on the total inter-trial interval. This pattern of performance was not found. There was also no selective effect of RCI on performance on task repetition trials.

The only other reliable effect on performance of varying the response-cue interval in this study was that the facilitative effect of preparation on RT was only reliable at long RCI. Mechanisms underlying preparation for a response are thought to be different from those affecting preparation for a task switch. One of these is variations in phasic arousal, which can reach a maximum about 500ms after a warning signal, so a short CTI may not allow maximum readiness for arrival and processing of the stimulus. In the same way, readiness for the cue might be reduced by a short RCI, impairing participants' ability to use the cue to maximise arousal during an ensuing long CTI to prepare, in turn, for arrival of the stimulus (see Meiran, Chorev et al. In press). Whatever the explanation of these unexpected findings, they do not alter the conclusion that there was no evidence for a time-dependent decay in task set activation (or inhibition) in this study.

Could 'TSI' have been responsible for task repetition benefits?

The present study showed clear progressive RT benefits of task repetition, but no effect of task repetition on a subsequent switch trial. There was also no evidence of asymmetric switch costs in the two subtasks that participants switched between, and none of carry-over inhibition from the previous trial affecting switching. This suggests that task set inertia, as operationalised in this experiment, did not influence performance on task switch trials. However, the predicted carry-over effect of task-irrelevant stimulus attributes on task repetition trials was found. Could TSI explain the interference-related gradual reduction in RT with task repetition demonstrated here? As described earlier, the TSI hypothesis was introduced specifically to explain task switch costs, in particular their dissipation over a number of trials (Allport, Styles et al. 1994). Its essential characteristic is persistence of a recently-performed and/ or competing task set, that builds up and dissipates gradually. In order to explain switch costs this must lead to switching to a new task being slower than performing the old one. The important point is that it operates at the level of the task set or rule, not at the level of the individual stimulus or response.

Experiments by Allport and Wylie carried out since the present study was completed

may shed some light on this (Allport and Wylie 1999; Allport and Wylie In press). These demonstrated positive and negative priming at the level of the task, as well as the stimulus-specific effect referred to earlier. Most importantly in the present context, it was clear that task 'reconfiguration' (a switch) was not complete in a single trial, as previous performance of a competing task affected task repeat trials, too. Unlike the slowing on task switch trials, this was thought likely to be due to an inihibitory carry-over effect. This was more marked when the current trials were incongruent than when they were neutral, and for incongruent trials appeared to decrease between trials 2-5 and 6-10 in a run. How might this relate to the present findings of a reduction in interference from the competing task with task repetition, and of benefits of a bivalent previous trial on task repetition trials (see **Figure 3.3** and **Figure 3.4**)? A lessening of the difference between RT on bivalent and univalent trials with task repetition is common to both studies, and may reflect the same underlying processes. If so, this would mean that the interference-related task repetition effect is the result of recent performance of the competing set, a kind of positive priming of task set that reduces (by whatever mechanism) with practice over a few trials.

Recent data from Mayr and Keele have also shown effects of inhibition at the level of the task rule, which the authors call 'backward inhibition' (Mayr and Keele In press). Switching *back* to a task performed two trials ago was found to be slower than switching to a third task. There was some evidence that this mechanism was contributing to the residual switch cost. Although these findings say nothing directly about task repetition performance, in the same study there was no reduction in the size of the effect between lags of 2 and 3 trials from one relevant switch to the other. That, and the fact that backward inhibition appeared to decline with time, over longer RCIs (unlike the sequential valence effects here), suggest that different types of inhibition may have been responsible, backward inhibition possibly being related to the time-based effects described by Meiran et al (in press). Clearly this is a matter for further experimentation.

The improvement in processing of interfering information in this study, where there previous trial was also bivalent, occurred over a single trial transition, and, as argued above, suggests inhibition of the now-irrelevant task. It is possible that this could have been responsible for the exogenous task repetition effect, that is, the progressive reduction in the effect of bivalent, compared with univalent, stimuli with task repetition. Although the formal comparison between task switch and repeat trials was

not reliable in the analysis of sequential valence effects, a previous bivalent stimulus did significantly improve task repetition performance overall.

However, a causal link has not been proven between the two findings. The claim is also weakened by the apparent lack of an effect of previous stimulus valence on task switch trials (discussed earlier), despite the increase in the effect of interference on a task switch. The hypothesis that inhibitory carry-over effects were responsible for the exogenous task repetition effect in the present study clearly requires direct experimental evaluation. These processes will receive further attention in the context of broader issues of task control in the next Chapter.

Implications for other models of task switching

To what extent can the involvement of inhibitory mechanisms in task switching and repetition be reconciled with accounts of task switching other than task set inertia? As already discussed, it is not clear what relationship, if any, Meiran et al's (in press) switching process that dissipates with time has to effects of stimulus valence on task switching and repetition in this study, or in Meiran et al's 3-component model.

Meiran (1999) accounts for the sequential effects he found of stimulus valence, not in terms of task set activation and inhibition, but as the sequelae of whether or not the 'stimulus task-set' on the previous trial was used or not. Preparatory switch costs are said to derive from focussing attention to a different dimension of the stimulus, 'reconfiguring the stimulus task-set'. It is implied that because with univalent stimuli attention does not need to be focussed, the stimulus task-set is not configured. Configuring the stimulus task-set for a different task on a subsequent switch trial is therefore quicker. It therefore seems that Meiran et al's account would predict that sequential stimulus valence effects are affected by preparation, but do not influence the effect of valence on task switching. The latter is contrary to the idea, put forward here, that common processes underlie both these types of valence effect. Such predictions can be tested, and the latter proposal will receive some further attention in the next chapter.

As described above, in Rubenstein et al's (in press) model of task switching, effects of task repetition can be related to the two different control processes, goal shifting and rule activation. The interaction of stimulus valence and task repetition would need to be explained by factors affecting rule activation. Where might carry-over effects from

previous trials fit in, if at all? The rule activation stage is specified as being essential where there is task-interference, but possibly not necessary for switching where there is not. Rubenstein et al suggest that proactive interference or TSI could affect rule activation. They also suggest that rule activation might begin only on arrival of the stimulus, as a result of attention to, or identification of, the relevant stimulus attribute (e.g. a picture or a word), explaining its independence from task cueing. If this were the case then operation-complexity effects, found in their study to interact with the effects of irrelevant characters, should influence the effect of previous trial valence on task repetition trials. If the process reflected in this is the one responsible for the interference-related task repetition effect, then operation-complexity would also be expected to influence the latter.

More general implications for task control

It can be seen from the above discussion that models of task switching, almost by definition, have not so far allowed for task control to take place on task repeat trials. However, the concepts of control processes used in these models can generally be adapted to accommodate a more flexible implementation of control, with the exception of Rogers and Monsell's stimulus-cued completion. The present study did not directly address the question of to what extent a task switch is 'special', aiming to demonstrate continuity, not dissociation, between task switching and repetition. This important general issue will be considered further in Chapter 6.

Executive control, interference and inhibition

How do the findings of this study relate to the issues of task control raised in the previous chapter? The comparison between exogenous task cueing, used in Experiment 2, and the endogenous cueing method used in Experiment 3, showed that cueing affected performance in a number of ways. Of interest here are the 'general' effects on performance of using cues to 'switch' and 'stay', rather than cues that specified the task directly by name. These effects were the ones not related to the time of arrival of the cue. Firstly, there was a greater effect of stimulus valence on both switch and repeat trials with endogenous cueing, particularly at short CTI. Secondly, performance of a concurrent high central executive load working memory task was found to reduce the task switch cost, but only for bivalent trials. This was because performance was slowed by the high CE load on bivalent task repeat trials.

Allport et al (1994) initially proposed TSI as an alternative to the executive control of switching, which in turn was assumed to be reflected in the ability to prepare for an upcoming task switch. However, an influence of carry-over effects from previous trials does not, in itself, imply that such effects are passive in nature, nor that they dissipate passively subsequently. Executive control could theoretically be involved either in the processing giving rise to the carry-over effects, or in the subsequent suppression of such effects. Similar opinions have recently been voiced by Allport and Wylie (in press).

Conclusions

The present study investigated task repetition, and has shown that its effects on RT can be progressive, and depend on more than one underlying process. Task repetition was found to improve RT more in the absence of task preknowledge provided by an informative cue. It also progressively reduced the effect of interference from task-irrelevant stimulus information, and there was evidence of inhibitory carry-over effects that might have been related to this reduction. At the end of the previous chapter, the possibility was raised that the reduction of switch costs with central executive load could have been mediated by disruption of inhibitory control. Could the latter two findings both relate to inhibitory executive control? The two studies reported in the next Chapter aimed to explore this issue further, by examining the effect of CE load on performance over several task repetitions.

Chapter 4

The central executive and task repetition performance

General introduction

The two experiments reported in this Chapter returned to the role of working memory in task switching, explored initially in Chapter 2. A dual task method was reintroduced, but with a focus on the effects of concurrent load on task repetition trial performance. In this way it was hoped that insights gained from the findings of Experiment 4 might help explain the effects of central executive load in Experiment 3, and allow some more definite conclusions to be drawn about the role of working memory in task control.

It was suggested earlier that the pattern of findings of Experiment 3 could be accounted for by involvement of the central executive in inhibitory control. Although the comparison with Experiment 2 showed some clear differences in performance with implicit as opposed to explicit task cues, not all of these were influenced by concurrent CE load. To recap briefly, preparation for a switch took longer with the implicit 'switch' or 'stay' cues than with the explicit task cues. With explicit cues, CE load affected overall speed and accuracy only. With implicit cues, however, it increased the amount of interference between tasks, and did so more on task repeat than on task switch trials. Switch costs were therefore *reduced* by CE load where task cueing was implicit, but only for bivalent stimuli. CE load still had no effect on preparation for a switch when implicit cues were used.

This raised the interesting possibility that performance on trials in which the task repeats in the presence of interference normally benefits from inhibitory control, at least when task cueing is implicit, but that CE load impairs that control. It had been assumed up until that point that the increase in switch costs found where stimuli are bivalent (e.g. Rogers and Monsell 1995) occurs because of 'extra' RT slowing on task switch trials. In Experiment 4, however, it was demonstrated that the slowing effect on RT from task-irrelevant information is *gradually* reduced over several successive

task repetitions. This appeared to be independent of task-preparation (preparation-independent task repetition effect). Carry-over effects were also found of the control of interference on a previous trial, which appeared to facilitate the same process on the current trial, if the task remained the same. Although this was not tested directly by that study, it was speculated that such carry-over effects might be associated with the cumulative suppression, at least over a few trials of task repetition, of the competing task rule. If the executive control of interfering information contributes to task repetition RT benefits, and thus overall 'switch costs', whether or not the suggested mechanism underlies it, this could account for the loss of switch costs in Experiment 3 with concurrent CE load. This was one possible account of the preparation-independent task repetition effect, and the influence on it of CE load. The main alternative explanation for this pattern of findings was short-term memory based, since incorrect responses on the current trial could have resulted from a difficulty remembering the task performed on the last trial. This, however, would not account for the disproportionate effect on task repetition trials. The experiments in the present chapter set out to distinguish between these possibilities, by replicating and extending the findings of Experiments 2, 3 and 4, analysing the performance only for participants who were sufficiently accurate.

Theoretical issues

If working memory were found to be involved in the control of interference during task repetition, this would have important implications both for cognitive models designed to explain switch costs, and for the understanding of control processes in working memory. Models of switching have tended to assume that, if executive control is involved, it will be involved in preparation for a switch (Allport, Styles et al. 1994; Rogers and Monsell 1995; Meiran 1996), rather than contributing to the preparation-independent component of the switch cost. Evidence for executive involvement in the control of interference in this context would therefore cause difficulties for Rogers and Monsell's and for Meiran's model. Allport and Wylie's current account, like Allport, Styles and Hsieh's (1994) original 'task set inertia' hypothesis, describes a number of relatively automatic processes, linked to conflict between tasks, that can contribute to switch costs (Allport and Wylie 1999; Allport and Wylie In press). Evidence, for example, of controlled inhibition of competing task rules could add to such a model. In terms of working memory, such evidence would be consistent with certain accounts that already specify WM involvement in inhibitory control (Engle, Conway et al. 1995);

Hasher, Rypma et al 1989). It would also link these accounts with that of Baddeley and coworkers (Baddeley 1996), providing a firmer basis for the central executive component of working memory as an independent control mechanism.

Experiment 5a: Explicit task cueing

Introduction

This study attempted to replicate the findings of Experiments 4 and 2, using explicit task cueing, and therefore serve as a baseline for examining effects of implicit task cueing in Experiment 5b. The low and high central executive load concurrent tasks used in Chapter 2 were included, so extending the scope of Experiment 2 to include the effects of load on successive task repetition trials. Cue-target interval was manipulated in this study, as in previous studies, to examine the effects of task-specific preparation. This was partly to replicate the findings of Experiment 4, that there are at least two processes contributing to the progressive RT benefits of task repetition. It was also included so that it could be established whether any effects of CE load obtained were independent of preparation, or not. Response-cue interval, however, was not manipulated as the decay of effects over time was not of interest in this experiment.

The inclusion criterion for participants by accuracy of performance on the tasks was set on the basis of the data from Experiments 2 and 3. In Experiment 2, with explicit task cueing, most participants' overall error rates were well within an upper limit of 5%, except for two, who made 5.1%. The average overall error rate was 2.3%. It was therefore felt that, with the stronger instructional emphasis on accuracy in the present study, it should be easy for participants to keep within the required criterion. However, it had been Experiment 3 in which poor accuracy had been thought possibly to account for the apparent impairment of task repeat trial performance. In this study the average overall error rate was 5.5%, with fifteen participants making more than 5% errors. It was hoped that enforcing comparable rates of accuracy in the present two studies would render the RT data easier to compare than the data from the earlier two experiments.

Predictions

Task switching and repetition

The first set of predictions concerned the replication of task repetition effects found in Experiment 4, and the effects that CE load would have on these. Progressive RT benefits of task repetition were predicted, that were diminished by preparation, but more marked on bivalent than on univalent trials, independently of preparation. Thus, with successive task repetitions, the effect of interference on RT would decrease, independently of the cue-target interval. It was expected that both low and high CE load would cause overall decrements in speed and/ or accuracy, rather than any specific effects on either task switching or repetition.

Carry-over effects of interference

The effects of previous trial stimulus valence on current trial performance were also to be examined in this study, as in Experiment 4. When the task repeated, RT for bivalent stimuli was predicted to be faster if the previous stimuli had also been bivalent, as in Experiment 4. RT on univalent trials was expected to be unaffected, suggesting that the interference effect on a given trial was reduced by previous-trial suppression of interference. When the task switched, no reliable effects of previous stimulus valence were expected, as in Experiment 4.

Method

Participants

24 undergraduate and graduate students from the University of Bristol, and members of a subject panel, took part in the experiment in exchange for cash or course credits. They were aged between 18 and 28 years. Three participants were excluded at the time of testing, for failing to comply with task instructions, and further participants included to replace them. Subsequently, three participants initially in the study were excluded because they did not meet the inclusion criterion of making less than 5% errors overall. Three more participants were then tested to replace these.

Task description

Participants were asked to switch unpredictably between the same picture and word classification sub-tasks as were used in Experiment 4 (for details, see Method section in previous Chapter).

The secondary tasks used were the 'low CE load' and 'high CE load' tone counting tasks used in Experiments 2 and 3 (see Method for Experiment 2).

Apparatus and stimuli

The apparatus and stimuli were the same as in Experiment 4. Also as in that study, the explicit task precues 'PIC' or 'WORD' were used.

Design

The experiment manipulated 4 main independent variables in a 5 x 2 x 3 x 3 repeated measures design. These were task switching/ repetition, cue-target interval, stimulus valence, and secondary task condition. These four variables and their levels were equivalent to those with the same names in Experiments 2, 3 and 4. The selection of the sub-task on each trial was random, as in Experiment 4. Counterbalancing procedures were also the same as in the earlier studies.

Procedure

The sequence of events on a particular trial for the primary task was the same as that in Experiments 2 and 3. The sub-task and other variables were determined randomly on each trial, as in Experiment 4, the probability of a task switch on each trial again being 0.25. The beeps for the secondary tasks were produced in the same way as in Experiments 2 and 3 (see Methods section for Experiment 2).

The experiment consisted of 18 blocks of 49 trials each, 6 blocks for each concurrent task condition. As in previous studies in this thesis, the first trial of each block was not recorded. Participants were encouraged to take short breaks at least every two blocks, and longer rests between conditions.

The practice procedure was the same as in the earlier studies reported here, except for the last of the four blocks. As in Experiments 2 and 3, this was an opportunity for

practice at task combination, but all participants in the present study performed the 'low CE load' secondary task during this block.

As explained in the Introduction, there was a concern in the present experiment with maintaining as high a level of accuracy on the primary (switching) task as possible. From inspection of error rates in earlier experiments, a cutoff of 5% errors overall was adopted, so that any participant who made more errors than this was excluded from the study, and a new participant included. Instructions to participants were based on those given in earlier studies, with additional emphasis on maintaining accuracy on the RT task, which was reinforced verbally.

Results

The data from the line and shape sub-tasks were combined in all analyses presented in this Chapter (see previous Chapter for comment on differences in sub-task performance). Effects not pertaining to the specific predictions of this study are reported but not considered in detail, unless they appear inconsistent with important findings in the literature. In the case of predictions concerning interference for RT, these concern primarily the effect of stimulus valence, and therefore the difference between *both* congruent and incongruent bivalent, and univalent trials. The previous studies in this thesis, as well as that of Rogers and Monsell (1995), have established that this is appropriate. Data for the former two conditions have therefore been combined when examining specific predicted effects, unless there is a particular reason to do otherwise. As for earlier studies in this thesis, error data will be reported briefly unless important effects are found.

Task switching and repetition

The first analysis looked at the effects of preparation and stimulus valence on task switching and repetition performance. It was expected that progressive RT benefits of task switching/ repetition would be found that were influenced, independently, by preparation and by valence. This would replicate the findings of Experiment 4. The effects of low and high CE load were also examined, although if the findings of Experiment 2 were replicated, no specific effects on task switching and repetition were expected in this study. These data would serve as a baseline for the comparison with Experiment 5b, where implicit task cues were used (see below). A repeated measures ANOVA was performed on median RTs and error proportions, with the following

factors: task switching/ repetition (switch trials, then 1st/ 2nd/ 3rd- 4th/ 5th- 8th task repetitions), cue-target interval (short/ long), stimulus valence (univalent/ bivalent-congruent/ bivalent-incongruent), and concurrent task (single task/ low CE load/ high CE load).

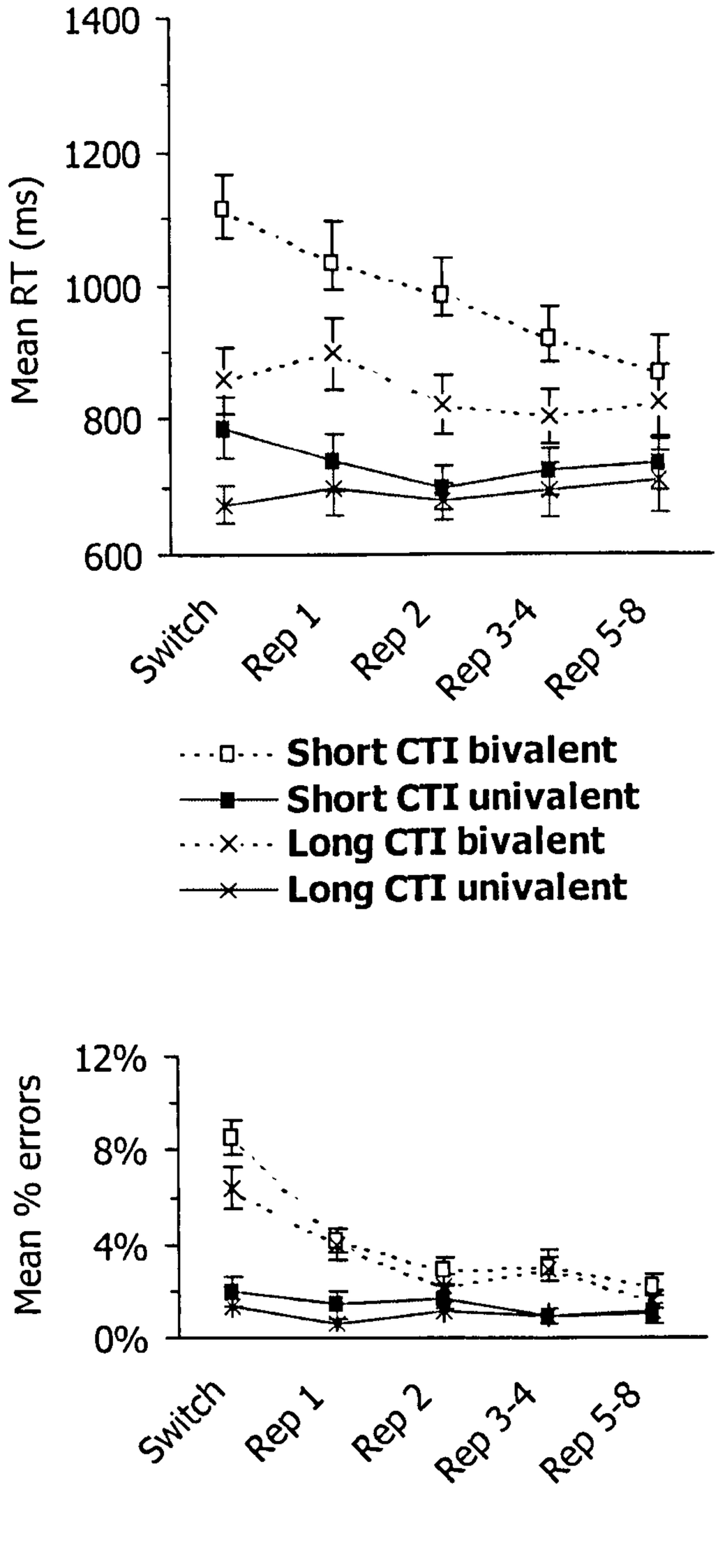

Figure 4.1. Shows the effects of task switching and repetition on RT and errors in Experiment 5a. The data for congruent and incongruent bivalent trials have been combined, in accordance with the main predictions. Error bars represent the standard error of the mean.

Figure 4.1 shows the effects of CTI and valence on task switching and repetition in Experiment 5a, for all concurrent task conditions together. It is clear from inspection of the data that the pattern of findings in Experiment 4 was broadly replicated.

Performance gradually improved over successive task repetitions, and (on the whole) performance was poorer on the switch trial, but improved more rapidly, at the short CTI and for bivalent trials. The results of the ANOVA are given in full. Follow-up analysis is then presented for effects involving task repetition, and effects involving load but not task repetition are examined next.

For RT, there were reliable main effects of task switching/ repetition ($F(4,92) = 14.15$, $p < 0.001$), of CTI ($F(1,23) = 42.42$, $p < 0.001$), and of stimulus valence, $F(2,46) = 99.02$; $p < 0.001$). The 2-way interactions of task switching and repetition with CTI ($F(4,92) = 8.20$, $p < 0.001$) and with valence ($F(8,184) = 4.23$, $p < 0.001$) were both reliable, but the 3-way interaction of task switching/ repetition with CTI and valence was not ($F(8,184) = 1.49$, n.s.). As in previous studies, there was also a significant interaction, for RT, of CTI and valence ($F(2,46) = 9.31$, $p < 0.001$). For errors, the only reliable effect was an interaction of task switching/ repetition with stimulus valence ($F(8,184) = 16.48$, $p < 0.001$).

Looking at the effects of concurrent load, there was a reliable main effect of secondary task condition on RT ($F(2,46) = 45.65$, $p < 0.001$), and a significant interaction with valence ($F(4,92) = 4.38$, $p < 0.005$). There were no other reliable effects of load, the 2-way interactions with task switching/ repetition ($F(8,184) = 1.66$, n.s.) and with CTI ($F < 1$, n.s.) being non-significant. None of the 3-way interactions involving CE load was reliable ($F < 1$, n.s. for all), and neither was the 4-way interaction ($F(16,368) = 1.32$, n.s.). For errors there were no reliable effects of CE load, with a trend only towards an overall increase in errors in the secondary task conditions (for main effect of CE load, $F(1,23) = 2.76$, $0.05 < p < 0.1$; see **Figure 4.1**).

Preparation effects on task switching and repetition

Follow-up tests were conducted on the interaction of task switching and repetition with CTI. As predicted, the simple main effect of task switching/ repetition at short CTI was reliable ($F(4,92) = 17.15$, $p < 0.001$), with a significant decreasing linear trend ($F(1,23) = 50.46$, $p < 0.001$). At long CTI, it was also reliable ($F(4,92) = 2.95$, $p < 0.05$), with a significant decreasing linear trend ($F(1,23) = 4.53$, $p < 0.05$). The influence of preparation on the task repetition effect was reflected in the fact that the linear trends in RT with task repetition at long compared with short CTI were reliably different ($F(1,23) = 21.54$, $p < 0.001$).

Interference effects on task switching and repetition

Looking in more detail at the interaction of task switching and repetition with stimulus valence, the data again confirmed predictions. The effect was more marked for bivalent stimulus trials, for which there was a reliable simple main effect of switching and repetition ($F(4,92) = 17.18$, $p < 0.001$), with a significant decreasing linear trend in RT ($F(1,23) = 73.71$, $p < 0.001$). For univalent trials, the simple main effect of task switching/ repetition was not reliable ($F(4,92) = 1.28$, n.s.).

For errors, the interaction of task switching/ repetition with valence was explained by the fact that the simple main effect of switching/ repetition was reliable for incongruent ($F(4,92) = 27.80$, $p < 0.001$) and for congruent bivalent trials ($F(4,92) = 2.70$, $p < 0.05$), but not for univalent trials ($F < 1$, n.s.). However, the only reliable differences between trial types were between incongruent and other trials, since the simple interaction effect of task switching/ repetition and valence was reliable for the comparisons of incongruent vs. congruent ($F(4,92) = 18.66$, $p < 0.001$), and incongruent vs. univalent trials ($F(4,92) = 18.86$, $p < 0.001$), but not for congruent vs. univalent trials ($F(4,92) = 1.27$, n.s.). There were also significant interactions in the linear task repetition trends for errors between incongruent and both congruent ($F(1,23) = 59.22$, $p < 0.001$) and univalent trials ($F(1,23) = 49.09$, $p < 0.001$).

The effects of CE load on performance

The effect of CE load on RT and errors according to stimulus valence are given in Table 4.1. RTs were generally slower in the high CE load than in the low CE load conditions, but performance did not differ much between the low CE load and single task conditions. Analysis of the simple interaction effect of load and valence was reliable for the comparison between low and high CE load conditions ($F(2,46) = 5.13$, $p < 0.01$) but not for the comparison of the single task with the low CE load condition ($F < 1$, n.s.). Further analysis of simple interaction effects then compared only bivalent with univalent trials. This also showed a reliable difference between the high and low CE load conditions ($F(1,23) = 38.31$, $p < 0.001$), but not between the low CE load and single task conditions ($F(1,23) = 1.92$, n.s.).

	Univalent stimuli		Bivalent congruent stimuli		Bivalent incongruent stimuli	
	RT (ms)	% errors	RT (ms)	% errors	RT (ms)	% errors
Single task	625	1.2	786	1.3	799	5.0
Low CE load	660	1.0	836	1.4	843	6.0
High CE load	857	1.4	1101	1.8	1118	7.3

Table 4.1. Summary of the effect of CE load on performance in Experiment 5a, and its interaction with stimulus valence. Means only are shown.

It can be seen from Table 4.1 that the corresponding error data can account for the trend towards a main effect of load, as well as the effects of stimulus valence already described.

Summary

This study broadly replicated the findings of Experiment 4 in terms of progressive benefits of task repetition. As before, these were more marked at short than long CTI, i.e. reduced by preparation for a specific task. They were also greater for bivalent than for univalent trials, over both cue intervals. However, the simple main effect of switching and repetition for univalent trials alone was not reliable, unlike in Experiment 4, so task switching/ repetition RT effects were *only* reliable for bivalent trials. Inspection of the data did not suggest a trend towards such an effect at short CTI.

Analysis of the effects of previous trial interference

As in Experiment 4, an analysis was carried out to see to what extent performance was affected by the suppression of interfering information on the previous trial. An improvement in task repetition trial performance was expected on current bivalent trials (congruent and incongruent considered together), where there had also been interference on the previous trial. CE load was not expected to influence this effect in the present study, because of the explicit method of task cueing. As no difference had been found between first and subsequent task repetitions in the earlier study, all task repetitions were considered together here. Otherwise, the analysis was carried out in the same way as in Experiment 4. A repeated measures ANOVA with the following factors was carried out on median RT and on error proportions: Task switching (task switch/ task repeat), stimulus valence on current trial (univalent/ bivalent), stimulus valence on previous trial (univalent/ bivalent), and secondary task load (single task/

low CE load/ high CE load). Only effects concerning previous trial valence will be discussed here. Because of the different predictions, subsidiary analyses were again planned to examine the effects of this variable on task switch trials and on task repeat trials, separately. The data are given in **Table 4.2.**

The overall main effect of previous trial valence was not reliable for RT, and nor were its 2-way interactions with load, with task switching and with current trial valence ($F < 1$, n.s. for all). There were, however, reliable 3-way interactions between task switching and previous and current trial stimulus valence ($F(1,23) = 10.34$, $p < 0.005$), and between CE load, task switching and previous trial valence ($F(2,46) = 3.76$, $p < 0.05$). The 3-way interaction of load with previous and current trial valence, and the 4-way interaction, were both non-significant ($F < 1$, n.s.). For errors, there were reliable 3-way interactions of secondary task load with task switching and previous trial valence ($F(2,46) = 6.98$, $p < 0.005$), and of load with current and previous trial valence ($F(2,46) = 4.40$, $p < 0.05$). There was also a reliable 4-way interaction

		TASK SWITCH		TASK REPEAT	
SINGLE TASK :		**Previous univalent**	**Previous bivalent**	**Previous univalent**	**Previous bivalent**
Univalent	RT in ms.	760 (57)	682 (36)	604 (21)	605 (21)
	% Errors	1.1 (0.9)	2.4 (0.9)	1.2 (0.4)	0.9 (0.3)
Bivalent	RT in ms.	856 (47)	899 (46)	754 (37)	734 (38)
	% Errors	7.6 (1.6)	6.0 (1.1)	2.4 (0.5)	2.0 (0.3)
LOW CE LOAD:					
Univalent	RT in ms.	714 (34)	684 (29)	626 (28)	653 (28)
	% Errors	1.6 (1.0)	0.6 (0.4)	2.2 (0.7)	0.8 (0.3)
Bivalent	RT in ms.	954 (50)	936 (52)	802 (45)	782 (35)
	% Errors	4.5 (0.9)	9.0 (1.0)	3.8 (0.6)	2.1 (0.3)
HIGH CE LOAD:					
Univalent	RT in ms.	918 (97)	905 (62)	848 (67)	814 (48)
	% Errors	2.4 (1.2)	1.5 (0.6)	0.6 (0.3)	1.6 (3.2)
Bivalent	RT in ms.	1086 (99)	1187 (62)	1128 (83)	1066 (71)
	% Errors	7.6 (1.3)	8.6 (1.1)	3.9 (0.7)	3.2 (0.4)

Table 4.2. Experiment 5a: Sequential stimulus valence effects. Shows the effects of previous trial valence on performance, depending on the valence of the current trial and on task switching. Mean RTs and error proportions are given, with the standard error of the mean in brackets.

($F(2,46) = 7.34$, $p < 0.005$). Findings for RT, and then for errors, will now be considered in more detail.

Overall effects of previous interference

As CE load had not been predicted to influence previous trial valence effects in this study, the first question was whether the 3-way interaction, for RT, of task switching with current and previous trial valence for RT, reflected the predicted pattern of performance and replicated the findings of Experiment 4. Across all concurrent task conditions, the simple interaction effect of current and previous trial valence was found to be reliable for task repeat trials ($F(1,23) = 4.68$, $p < 0.05$), and there was a trend towards an interaction for task switch trials ($F(1,23) = 3.89$, $0.05 < p < 0.1$). On task repeat trials, it was expected that a previous bivalent trial would be associated with faster RTs where the current trial was also bivalent. This prediction was confirmed by the finding of a significant simple main effect of previous trial valence on current bivalent trials only ($F(1,23) = 6.30$, $p < 0.05$; for current univalent, $F < 1$, n.s.). On task switch trials, previous trial valence had not been expected to influence performance significantly, and inspection of the data confirmed this, showing that the trend towards an interaction between previous and current trial valence here did not result from any theoretically interesting and reliable effects.

Effects of concurrent load

The 3-way interaction of concurrent task interaction with previous valence and task switching for RT had been unexpected, and this was investigated next. It was found to be due to a difference between the high and the low CE load conditions, since the simple 3-way interaction effect of these factors was reliable when the low and the high CE load conditions were compared ($F(1,23) = 5.26$ $p < 0.05$), whilst the single task and low CE load conditions did not differ ($F < 1$, n.s.). Examination of the two secondary task conditions separately suggested that concurrent task influenced previous trial valence effects only in the high CE load condition. In that condition only, there was a reliable simple interaction effect of task switching and previous trial valence ($F(1,23) = 4.59$, $p < 0.05$; for low CE load, $F(1,23) = 1.05$, n.s.).

Although the simple main effects of previous trial valence in the high load condition were not individually reliable for task switch ($F < 1$, n.s.) and task repeat trials ($F(1,23) = 2.57$, n.s.), the trends were in opposite directions for these two trial types. On task

switch trials, a previous bivalent trial slightly increased RT, and on task repeat trials, it slightly reduced it. However, when the task repeated, the predicted effects of previous valence (which were made irrespective of CE load) depended on the valence of the current stimulus. Post hoc tests were therefore carried out for these trials, looking at the simple main effects of previous trial valence in the high CE load condition only. These did not reach statistical significance, but there was a trend towards an effect on current bivalent trials ($F(1,23) = 5.27$, $0.025 < p < 0.05$; adjusted $\alpha = 0.025$), along with no sign of one on univalent trials ($F < 1$, n.s.), does at least suggest that there was no substantial divergence here from the predicted *pattern* of findings, and from those of Experiment 4. This therefore suggested that the interaction of low versus high load concurrent task with task switching and previous trial valence arose for two reasons, both affecting task repeat trials. Firstly, instead of a slight (but non-significant) RT slowing on univalent trials if the previous trial was bivalent, there was a slight (but non-significant) RT speedup. Secondly, the effect of previous trial valence on current bivalent trials was numerically larger in the latter than in the former condition.

So most importantly, there was no sign of a *loss* of the major sequential effect of stimulus valence with concurrent load (on current univalent trials), which is what would be expected if CE load was impairing the processes responsible (see **Table 4.2**). The pattern of previous valence effects in the experiment as a whole is illustrated in **Figure 4.2**, where the data have been collapsed over secondary task condition.

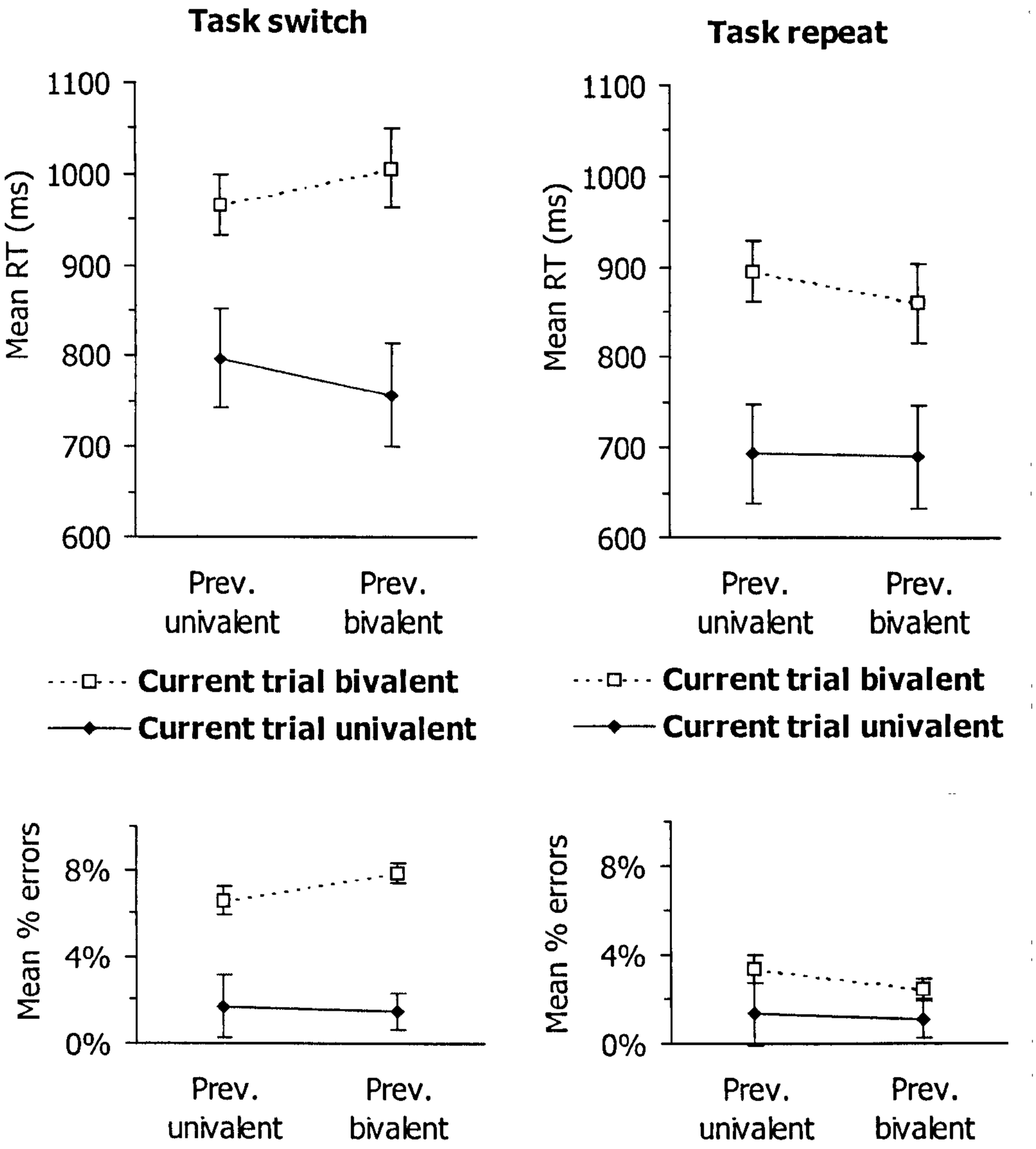

Figure 4.2. The effects of previous trial stimulus valence in Experiment 5a, for all concurrent task conditions together. Errors are given for comparison with RT (see text for details). Error bars represent the standard error of the mean.

Concurrent load and error rates

The 4-way interaction for errors of secondary task condition with task switching, current and previous trial valence, like the load effect on RT, was unexpected. Unlike the latter, however, it was due to a difference in performance between the single task and the low CE load condition, for which comparison the simple 4-way interaction effect was reliable ($F(1,23) = 16.33$, $p < 0.001$). There was no significant difference between the low and high CE load conditions in this respect ($F < 1$, n.s.). The simple 3-way interaction effect of task switching, current valence and previous valence was

not reliable in the single task condition ($F(1,23) = 2.54$, n.s.), but was reliable in the low CE load condition ($F(1,23) = 10.86$, $p < 0.005$). Looking at the pattern of errors in the low CE load condition in more detail, there was a significant simple interaction effect of current and previous trial valence for task switch trials ($F(1,23) = 11.53$, $p < 0.005$), but not for task repeat trials ($F < 1$, n.s.). On task switch trials, a bivalent compared with a univalent stimulus reliably increased the error rate only when there was also a previous bivalent trial (for simple main effect of previous valence, $F(1,23) = 19.68$, $p < 0.001$). When the current stimulus was univalent, this effect was not significant ($F < 1$, n.s.). Inspection of the data in **Table 4.2** shows that the pattern of performance on switch trials in the high CE load condition was broadly similar to this, whilst the trends were in the other direction in the single task condition.

Summary

There were two main questions addressed by this analysis. The first was whether the effects of previous trial stimulus valence shown in Experiment 4 would be replicated. There, previous bivalent stimuli were associated with faster RTs on current bivalent task repeat trials. The RT data fitted with this expectation, and although performance in the high CE load condition was slightly different, the difference appeared to be due mainly to different trends on current univalent trials. This was not of major theoretical importance, although performance strategies that could possibly have affected these trials will be considered later on. The predicted effect, on bivalent task repeat trials, was if anything slightly *more* marked with a high CE load. CE load did not affect task switch performance for RT.

The second question addressed by this analysis was whether concurrent load would influence these sequential effects of interference between tasks. The expectation was that they would not, as explicit task cueing was used here. This was largely supported by the RT data, as explained above. The error data, however, showed that previous valence effects differed between the single task and low CE load conditions. Participants made more errors in the latter on bivalent task switch trials preceded by another bivalent trial. There appeared to be a similar, though non-significant, pattern in the high CE load condition, but no hint of such an effect in the single task blocks. This had not been predicted, and was not the case in Experiment 4. Its implications will be considered in the next section.

Discussion of Experiment 5a

Effects of task repetition

The demonstration of progressive RT benefits of successive task repetitions in this study has replicated the findings of Experiment 4. This was most clear cut for the relationship between task repetition and preparation, with a marked decrease in the effect at long compared with short CTI, i.e. where there was an opportunity to prepare for the particular task. Task repetition also affected performance more on trials where there was interfering information from the other task than on trials where there was not. However, there was some discrepancy with Experiment 4 in that there was no evidence in the present study that repetition of the same task improved performance *at all* on univalent trials. In the earlier study, there was a reliable overall task repetition effect for univalent trials, which appeared to be due to performance at short CTI. In other words, the influences of preparation and interference on task switching/ repetition appeared to be independent. The current findings, however, suggest that repetition effects are *only* present for bivalent trials, but are still reduced by preparation. If this is the case, the influences of preparation and interference cannot be seen as independent. However, it should be noted that the 3-way interaction of task switching/ repetition with CTI and valence failed to reach significance in the present study. Also, despite this apparently discrepant finding, it should be noted that in other studies (including Experiments 1 to 4) of reliable switch costs/ repetition benefits have always been found for univalent trials at short cue-target or response-stimulus intervals (e.g. Rogers and Monsell 1995; Meiran 1996).

A further possibility requires some consideration, however, before the discrepant finding is ascribed to chance. In the previous Chapter, it was suggested that, since the univalent stimuli were easily disinguishable from the bivalent stimuli, participants might have opted effectively to switch between this and the picture and word (bivalent stimulus) 'subtasks'. Since an analysis was carried out of previous valence effects, it is possible to address this possbilitiy to some extent using the present data. Assuming that this, like switching between the original subtasks, took up more processing time than repeating the 'task', what would be predicted? Firstly, there would not be a switching/ repetition effect on univalent trials, since their *effective* switch or repeat status would depend instead on the valence of the preceding trial. With a '3 tasks' strategy, performance on univalent trials where the previous trial was univalent would

be equivalent to a task repetition, whether or not there was a picture ←→ word switch. Likewise, all univalent-bivalent and bivalent-univalent transitions would be equivalent to a switch. Bivalent-bivalent could be either a switch or a repetition, depending on the picture and word subtasks. **Table 4.3** shows the way that this

	Current trial type	Original predictions when previous bivalent trial	Trend introduced by '3-task' strategy	Pattern observed in Expt. 4
Task repeat	Univalent	Nil specific, slowing found*	Slowing	No sig. effect
	Bivalent	Speedup	Speedup	Speedup
Task switch	Univalent	Slowing, but nil found*	Slowing	No sig. effect
	Bivalent	Slowing, but nil found*	No difference	No sig. effect

Table 4.3. Assessing likelihood of '3-task' strategy, with univalent picture *and* word trials counting as a single '3rd task'. Predicted findings for RT are given first for switching between picture and word tasks only, then compared with the 3-task predictions. *This refers to the fact that not all of the predictions originally made for Experiment 4 received support from the data. See text for more details.

would affect the predictions for the previous valence analysis. The 3-task strategy suggested predicts RT slowing with a previous bivalent trial, on current univalent trials, whether the task switches or repeats. Error effects are not mentioned but would be expected to be in the same directions as RT effects.

The present data do not support these predictions. On task switch trials, there was in fact quite a different pattern, at least under concurrent task conditions, since the increased error rate was found on bivalent trials, with little effect being present on univalent trials (see **Figure 4.2**). Of course, the fact that the 3-task strategy did not make a major contribution to these findings does not preclude it occurring to some extent. It is noted that the variance in RTs was greater on univalent trials than on bivalent trials, despite smaller means. This could have been due to some variation in strategy on these trials, and is consistent with the '3-tasks' suggestion. This issue will be considered again in Chapter 6. Previous valence effects in this study are considered further below with respect to the predictions about CE load.

A final point concerns the residual switching/ repetition cost, after preparation, in the present study. This was was clearly absent for univalent trials, since the simple main effect of switching and repetition for these was not reliable over *both* cue intervals. It was, however, present when bivalent trials were included, as indicated by the simple main effect of switching and repetition at long CTI[2]. In Chapter 1 it was suggested that residual switch costs might be absent with explicit task cues, but present with implicit ones. The present findings are not consistent with this. The data from Experiment 5b will, however, be compared to these, in case a relative difference is found.

Load effects on task repetition

The results of this study clearly showed that neither the low nor the high CE load concurrent task significantly influenced the progressive performance benefits of task switching and repetition. This replicates the findings of Experiment 2, in which there were no reliable effects of the same concurrent tasks on switch costs, and extends them to a different pair of sub-tasks and longer runs of task repetition trials. However, as in the earlier study, the secondary tasks did affect performance overall, in the high CE load condition. High, as opposed to low, CE concurrent task load was found to increase the amount of interference for RT from task-irrelevant stimulus attributes. This was similar to the pattern of findings in Experiment 2, and if anything, more marked. As noted in Chapter 2, however, this finding is not interpretable alone, since the CE load could have affected any processing stage in task performance, such as stimulus encoding. Further comment therefore awaits the comparison with Experiment 5b, and any effects involving task cueing.

Carry-over interference effects

The effect of previous trial valence on RT in this study also replicated the findings of Experiment 4, both because task repetition trials were affected in a similar way, as predicted, and because task switch trials were not reliably affected. Secondary task load had no significant effect on these phenomena. However, the error data showed an influence of previous stimulus valence on current *switch* trial performance, where

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[2] When the single task was analysed separately, the same pattern emerged, although the simple main effect of task switching and repetition was not quite reliable ($F(4,92) = 2.39$, $0.05 < p < 0.1$).

the switch trial was bivalent. In this situation, more errors were made than where the previous trial had been univalent. This effect, although only reliable in the low CE load condition, also appeared to be present to some extent in the data from the high load condition. This suggested that it was likely to be the result of demands common to both concurrent tasks, such as articulation, or storage of phonological material. As the comments above indicate, it cannot be explained by participants employing a '3-task' strategy only in the dual task conditions. The implications of this finding will be considered in more detail in the General Discussion, in the light of the findings of Experiment 5b.

Experiment 5b: Implicit cueing and task repetition

Introduction

This study was designed specifically for comparison with Experiment 5a, to examine further the differences in task switching and repetition performance between implicit and explicit task cueing, and the effects on this of the same concurrent tasks. Whilst Experiment 5a, like Experiments 2 and 4, used explicit cues on each trial that named the task to be performed, the present study, like Experiment 3, used implicit cues that instructed participants to 'switch' or to 'stay'. The previous study largely replicated the findings of Experiment 4, with respect to progressive task repetition effects. One aim of the present experiment was to see whether these findings extended to implicitly-cued task repetition, and it was expected that they would. Preparation had influenced switch costs reliably in Experiment 3, as in Experiment 2, and therefore a robust preparation-dependent task repetition effect was predicted here. Although stimulus valence had not affected switch costs as expected in Experiment 3, this was thought to be because of the poor level of accuracy obtained by the participants in that study, an impression that was supported by a strong trend towards a parallel effect in the error data. This was part of the reason that the present study attempted to adopt a strict accuracy criterion for inclusion, of 5% errors overall. It was hoped that the findings would thus be more comparable with those of Experiment 5a than those of the pair of earlier studies had been.

CE load and inhibitory control

The other aim of the restriction and different instructional emphasis here was to allow two possible interpretations of the CE load effects in Experiment 3 to be distinguished from one another. High CE load was found to slow RT on task repetition trials only, and only for bivalent stimuli. If this was secondary to participants forgetting which task they were doing, which would be reflected in a high rate of incongruent trial errors, then successful prioritising of primary task accuracy by a subsequent group should produce a similar pattern of task repetition performance to Experiment 5a, and no influence of CE load. If, however, the pattern found in Experiment 3 was the result of a specific impairment of inhibitory control by the central executive, it should still be evident if performance was more accurate. The predictions are listed in detail below.

A further, and related, aim here was to replicate the findings of Experiments 4 and 5a with respect to the sequential effects of stimulus valence, and to determine whether previous interferece from the other task also affects performance with implicit task cueing. The broader context of this was an exploration of the possibility that such effects result from the activity of executive inhibitory control. It was proposed that, in Experiment 3, the loss of the task switch cost/ repetition benefit on bivalent trials under high CE load was due to an impairment in such a mechanism. When Experiment 4 revealed a progressive reduction of interference between tasks with task repetition, this was consistent with a carry-over effect of an inhibitory control process. If the two findings were related in this way, then under high CE load conditions there should be impairment of the task repetition benefit on bivalent trials, along with an absence of the sequential effects of valence found in Experiment 4.

Implicit cueing and task preparation

Finally, this study set out to replicate and extend the difference found in Experiments 2 and 3 between preparation for a switch with explicit and with implicit task cues. The switch cost at short CTI had been found to be greater in Experiment 3, suggesting that the 'endogenous' component of switching took longer with implicit cueing. Since Experiment 4 has since demonstrated that this task-specific preparation affects task repeat as well as task switch trials, the prediction in the present study was that such preparation would be slower than in Experiment 5a, over several task repetitions. If this were the case, rather than short CTI being slower with implicit cueing *only* on task switch trials, it would constrain the possible interpretations of the effect offered in Chapter 2. It was important to know whether the latter was because of extra processes taking place with endogenous/ implicit task cues at the time of a switch, suggesting control specifically of a task switch, or because of processing also affecting task repetition trials.

Most of the predictions depended upon analysis of Experiment 5b data alone, whilst a few demanded a formal comparison with Experiment 5a. The former will be given first, and then the latter. Results will be described and discussed separately, briefly, before moving to a general discussion of both studies in this Chapter.

Predictions for Experiment 5b

Task switching and repetition

As in Experiment 5a, the first set of predictions concerned task switching and repetition performance. The baseline (single task) pattern of performance was expected to be qualitatively similar in the present study. A reliable progressive RT benefit of task repetition was again expected, more marked at short than at long CTI, and more marked for bivalent than for univalent trials. No effect of either concurrent task on the preparation-dependent task repetition effect was expected. This would replicate earlier findings that working memory load did not affect preparation for a particular task, whichever cueing method was used. The most important prediction, however, concerned the influence of a CE load on task repetition effects on bivalent trials. It assumed that, when task cueing is implicit, the RT *benefit* of task repetition on bivalent trials relies upon the active inhibition of competing task rules by the central executive of working memory. It was therefore predicted that in the present study the task repetition benefit on bivalent trials would be impaired in the high CE load condition. This impairment was not expected to be affected by preparation.

Carry-over effects of interference

The rationale for the prediction here of a concurrent task influence on previous valence effects, on task repetition trials, has been given above. It was not possible to make principled predictions for the effects of previous trial valence on task switch trials, because none would have been made on the basis of the findings of Experiments 2, 3 and 4, and the findings of Experiment 5a in this respect had been unexpected. However the similarity, or difference, between the present and the latter study were clearly of interest in interpreting this result. The same carry-over effect of previous trial stimulus valence was predicted here as for Experiment 5a, on task repetition trials. Under high CE load conditions, this effect was expected to be absent, or impaired.

Comparing Experiments 5a and 5b

The findings of Experiments 2 and 3 suggested that task-specific preparation takes longer with implicit task cues than with explicit task cues. Looking at several successive task repetitions, a reliable progressive RT benefit of task switching and repetition was again expected, but at short CTI this was predicted to be more marked

than the effect in Experiment 5a. No reliable differences in the effects of valence on task switching and repetition between the two studies were expected (under baseline conditions, i.e. single task). The predicted differences under CE load were expected to be clear from the separate analysis of the two studies. However, an increase in the *overall* effects of stimulus valence on RT was expected in Experiment 5b compared to 5a, particularly in the high CE load condition, as for Experiments 3 and 2.

It was also expected that the interaction of CTI and stimulus valence would be more marked with implicit cues, in Experiment 5b, than in Experiment 5a. This prediction did not concern task switching and repetition, but the replication of an incidental finding from Chapter 1. Interference effects were expected to be greater at short CTI in both studies, but this would be more marked with implicit task cueing.

Method

Participants

There were 24 participants in this study, recruited from the same sources as the earlier studies. They were aged between 18 and 39[3] years. None had participated in either of the previous two experiments.

As testing proceeded, it became clear that participants found it extremely difficult to comply with the requirement that they make no more than 5% errors overall in the switching task. By the time 24 had been tested, only nine had kept within this criterion, and fifteen had made more than 5% errors. There was therefore a choice between including only a minority of the available population in the study, or adjusting the criterion. Since the very difficulty of the task combinations had potential theoretical implications (see later), the latter option appeared preferable. An inclusion criterion of less than 10% errors was therefore adopted for this study, a choice which received some support from the observation that this meant the exclusion of three participants, the same number as excluded for this reason in Experiment 5a, and the fact that the 3 appeared to form a distinct outlying subgroup. Three more participants were therefore tested to replace them.

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[3] This is known from general inclusion criteria. Unfortunately, the original data-sheets were mislaid.

Tasks and apparatus

The only difference between Experiment 5a and the present study was in the method of task cueing. This was the same as that employed in Experiment 3, with cues before each trial instructing participants to 'SWITCH' or 'STAY'.

Design

This was the same as in Experiment 5a.

Procedure

This was also the same as in Experiment 5a. The action taken in terms of inclusion criteria has been described above (see 'Participants').

Results

Task switching and repetition

The same approach to data analysis was used for this study as for Experiment 5a. The first analysis looked at the effects of preparation and interference between tasks on task switching and repetition performance, and the influence of low and high CE load on these. With implicit task cues, progressive RT benefits of task repetition were expected that would again be influenced by preparation and by stimulus valence. The latter was expected at least in the baseline condition. High CE load was expected to increase the amount of interference between tasks, but particularly on task repetition trials, leading to a loss of the interference-related switching/ repetition benefit. A repeated measures ANOVA was performed on median RTs and error proportions, with the following factors: task switching/ repetition (switch trials, then 1^{st}/ 2^{nd}/ 3^{rd}- 4^{th}/ 5^{th}-8^{th} task repetitions), cue-target interval (short/ long), stimulus valence (univalent/ bivalent-congruent/ bivalent-incongruent), and concurrent task (single task/ low CE load/ high CE load).

The mean RT and error data for Experiment 5b are illustrated in **Figure 4.3,** for all

secondary task conditions together. It can be seen that, as expected, performance improved gradually over successive task repetitions, and this was more marked at short than at long CTI. The RT data do not, however, appear to show a more pronounced repetition effect for bivalent trials, although this (predicted) pattern is evident in the error data.

The results of the overall ANOVAs will first be presented, then data explored further, and follow-up analysis given, for potentially important effects. As for the previous experiment, only those directly relevant to the evaluation of predictions will be considered in detail, and follow-up tests for RT involving valence effects will generally compare only bivalent with univalent trials.

RT data

The analysis of variance for RT revealed significant main effects of task switching/ repetition ($F(4,92) = 47.64$, $p < 0.001$), of CTI ($F(1,23) = 181.87$, $p < 0.001$), and of valence ($F(2,46) = 86.77$, $p < 0.001$). There were also reliable interactions of task switching and repetition both with CTI ($F(4,92) = 56.94$, $p < 0.001$) and with valence ($F(2,46) = 2.55$, $p < 0.05$), all as predicted. Unexpectedly, there was also a reliable 3-way interaction of these factors ($F(8,184) = 2.04$, $p < 0.05$).

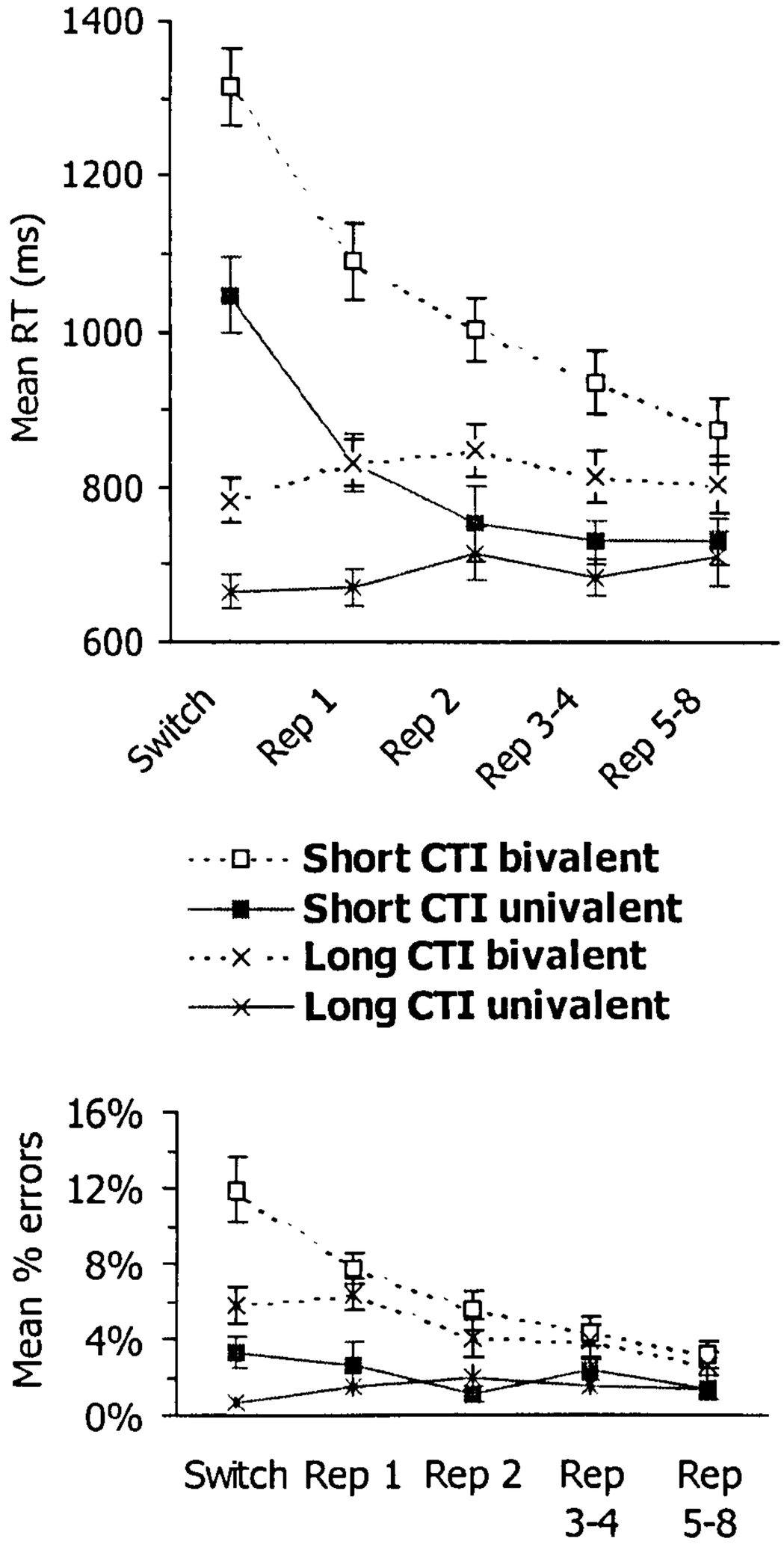

Figure 4.3. Shows the effects of task switching and repetition on RT and errors in Experiment 5b. The data for all bivalent trials have been combined, in accordance with the main predictions. Error bars represent the standard error of the mean.

Turning to the effects of secondary task load, as well as showing a reliable main effect ($F(2,46) = 51.47$, $p < 0.001$), this factor interacted significantly with both CTI ($F(2,46) = 4.15$, $p < 0.05$) and with stimulus valence ($F(4,92) = 4.47$, $p < 0.005$). The latter finding was expected, as was the reliable 3-way interaction of load, task switching/ repetition and valence ($F(16,368) = 2.11$, $p < 0.01$). In addition, there was some trend towards a 2-way interaction of CE load with switching and repetition ($F(8,184) = 1.96$, $0.05 < p < 0.1$). as predicted, the 3-way interaction of load with switching/ repetition and CTI was non-significant ($F < 1$, n.s.), as was the 4-way interaction ($F < 1$, n.s.). There was a significant overall interaction of CTI with stimulus valence ($F(2,46) = 13.59$, $p < 0.001$) as predicted and found in previous studies, but no

reliable 3-way interaction of load with these two factors ($F(4,92) = 1.16$, n.s.).

Error data

For errors, there were significant main effects of task switching/ repetition ($F(4,92) = 14.09$, $p < 0.001$), of CTI ($F(1,23) = 29.80$, $p < 0.001$), and of valence ($F(2,46) = 47.72$; $p < 0.001$). Task switching and repetition also interacted reliably with both CTI ($F(4,92) = 6.96$, $p < 0.001$) and with stimulus valence ($F(8,184) = 11.07$, $p < 0.001$). As for RT, these findings had been predicted, but the reliable 3-way interaction of task repetition these two factors had not ($F(8,184) = 4.47$, $p < 0.001$).

There was a reliable main effect of secondary task load ($F(2,46) = 12.73$, $p < 0.001$), an interaction of this factor with stimulus valence ($F(4,92) = 12.21$, $p < 0.001$), and some trend towards a 3-way interaction with task switching/ repetition and valence ($F(16,368) = 1.50$, $0.05 < p < 0.1$). No other effects were reliable involving this factor. The important effects will now be considered in more detail.

Task repetition effects, preparation and interference

RT findings

Inspection of **Figure 4.3** suggests that successive task repetitions led to more rapid improvements in RT for at short than at long CTI, as predicted. The 3-way interaction between task switching/ repetition, CTI and stimulus valence had not been predicted, however. This was analysed further, and the simple interaction effect of switching/ repetition with valence found to be reliable at short CTI ($F(8,184) = 2.88$, $p < 0.01$), but not at long CTI ($F(8,184) = 1.32$, n.s.). At long CTI, there was a reliable simple main effect of task switching/ repetition ($F(4,92) = 2.57$, $p < 0.05$), but this did not represent a progressive RT speedup with task repetition, since the linear trend in this effect was not significant ($F(1,23) = 1.05$, n.s.). Looking at performance at short CTI, the interaction of switching and repetition with valence, a comparison of bivalent and univalent trials, suggested that this contributed to the effect, as predicted ($F(4,92) = 2.51$, $p < 0.05$). The simple main effect of task switching and repetition was reliable for univalent trials ($F(4,92) = 25.78$, $p < 0.001$), with a significant linear trend ($F(1,23) = 103.75$, $p < 0.001$) as well as for bivalent trials ($F(4,92) = 56.49$, $p < 0.001$), again with a significant linear trend ($F(1,23) = 134.73$, $p < 0.001$). There was also a reliable interaction between the linear task repetition trends in RT for the two trial types

($F(1,23) = 5.81$, $p < 0.05$). Inspection of **Figure 4.3** shows that, as expected, the slope of the RT speedup with task repetition was more *more* marked for bivalent than for univalent trials.

Error findings

The error data followed a somewhat different pattern with respect to stimulus valence. It can clearly be seen from **Figure 4.3** that error rates for bivalent trials were higher at short than at long CTI when the task switched, and then decreased more rapidly over several task repetitions. Although the graph does not show the difference in performance between congruent and incongruent bivalent trials, this difference is relevant for the error data here, and the data were therefore analysed accordingly. The 3-way interaction of task switching/ repetition with CTI and stimulus valence for errors was the result of a reliable simple interaction effect of preparation and task switching/ repetition for incongruent trials ($F(4,92) = 6.99$, $p < 0.001$), but not for congruent ($F(4,92) = 1.08$, n.s.) or univalent ($F(4,92) = 1.99$, n.s.) trials. The simple main effect of switching and repetition was reliable for incongruent trials at both short CTI ($F(4,92) = 18.31$, $p < 0.001$) and long CTI ($F(4,92) = 3.82$, $p < 0.01$). There were reliable decreasing linear trends in error rates at both short ($F(1,23) = 45.86$, $p < 0.001$) and long ($F(1,23) = 12.76$, $p < 0.005$) CTI, but there was also a significant difference between these two trends ($F(1,23) = 29.66$, $p < 0.001$).

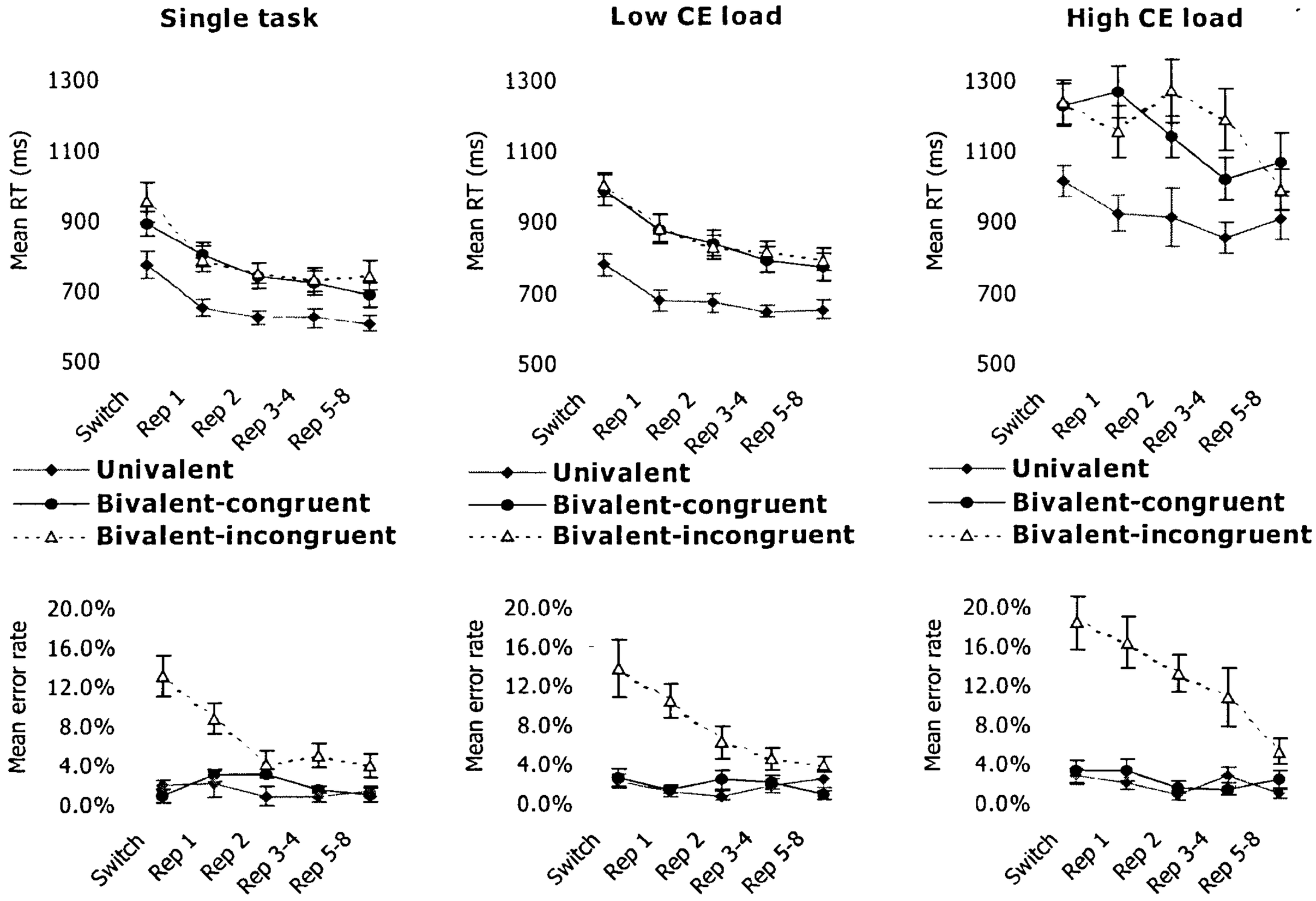

Figure 4.4. The effects of task repetition in Experiment 5b, depending on stimulus valence and response-congruence, for the three concurrent task conditions. Error bars represent the standard error of the mean.

CE load effects on task switching and repetition

Potentially the most important finding for RT was the interaction of CE load with task switching/ repetition and valence, because of the prediction that high CE load would reduce the RT benefits of task repetition on bivalent trials. The data are shown in **Figure 4.4.** However, further analysis of the 3-way interaction did not support this explanation. The relevant effect was indeed in the high CE load condition, but there was a reliable interaction of task switching/ repetition and valence *only* here ($F(8,184) = 2.66$, $p < 0.01$), and not in the single task ($F(8,184) = 1.02$, n.s.) or in the low CE load ($F(8,184) = 1.36$, n.s.) conditions. Further tests revealed that there was no significant difference between task repetition performance on univalent and bivalent trials in the high CE load condition ($F(4,92) = 1.94$, n.s.). This suggested that the cause of the interaction, as well as being unexpected, was not theoretically important. Inspection of the data suggests that the 3-way interaction probably resulted from two 'crossovers' between trends for congruent and for incongruent trials, from the 1st and the 2nd, and the 3rd- 4th and the 5th- 8th task repetitions (see **Figure 4.4).**

CE load effects not involving task switching/ repetition

Given the reason for the above 3-way interaction, the lower order effects of CE load here are of more interest. The data are summarised in **Table 4.4** for the interaction of concurrent task with stimulus valence. It appears that for RT, this was due to an increase in the effect of incongruent irrelevant attributes in the high CE load condition compared with the low CE load condition.

	Univalent stimuli		**Bivalent congruent stimuli**		**Bivalent incongruent stimuli**	
	RT (ms)	% errors	RT (ms)	% errors	RT (ms)	% errors
Single task	654	1.8	769	1.3	792	6.9
Low CE load	686	1.7	855	1.9	864	7.8
High CE load	921	1.9	1143	2.4	1166	12.9

Table 4.4. Summary of the effect of CE load on performance in Experiment 5b, and its interaction with stimulus valence. Means only are shown.

Further analysis of the RT data is deferred until this experiment is compared with Experiment 5a, however, since such effects are mainly of interest in the context of differences according to task cueing method. As explained in Chapter 2, the reason for this is that CE load could influence any stage of processing that varies with the

manipulation of stimulus valence. There is nothing here at present to indicate that it affected processing of an interfering task rule rather than, for example, a more complex visual stimulus.

As far as errors were concerned, follow-up tests showed that the interaction of secondary task load and stimulus valence was reliable for the comparison of low versus high CE load ($F(2,46) = 13.76$, $p < 0.001$), but not for single task versus low load ($F < 1$, n.s.). This was because high CE load increased the error rate on incongruent trials (for simple main effect of load, $F(1,23) = 20.93$, $p < 0.001$; for congruent trials, $F(1,23) = 1.20$, n.s.; for univalent, $F < 1$, n.s.). Because of the trend towards a 3-way interaction of load and valence with task switching and repetition, the data were inspected to see whether there were any potentially important differences in incongruent trial performance over successive repetitions according to secondary task load. Of interest in this context would have been signs that a relative increase in error rates in the high CE load condition occurred predominantly on task repetition trials, in particular 'later' task repetitions. However, the data clearly showed that any trend was in the opposite direction, towards relatively greater increases in error rates on task switch and 'early' task repetition trials.

The interaction of CE load with CTI is irrelevant to the predictions of the present study, and so will not be considered further.

Summary

The influence of preparation on task switching/ repetition in this study was broadly equivalent to that found in the previous study. Progressive RT benefits of task repetition were again found at short CTI, and these were reliably reduced at long CTI. Unlike in the previous study, there was *no* reliable RT switching and repetition effect at long CTI, but there was a reliable decreasing linear trend in error rates on incongruent-bivalent trials. Stimulus valence did not have the predicted effect on task repetition performance at short CTI, either, and there was some evidence of a *less* marked RT speedup for bivalent trials. However, this was accompanied by a more marked reduction of error rates with task repetition at short than at long CTI, for bivalent-incongruent trials. This suggests that an interference-related progressive task repetition effect was still present at both cue intervals, but did not reveal itself in the RT data because of the poorer response accuracy overall. The error data also suggest that this effect was somewhat more marked at short CTI, perhaps offsetting the

opposite direction trend in the RT data. It is also noted that at short CTI there was a clear task repetition effect for univalent trials, unlike Experiment 5a, but like Experiment 4.

The effects of concurrent task load, on the other hand, were fairly clear-cut, in that there was no evidence to support the predicted loss of task repetition effects for bivalent trials under conditions of high CE load. There was no important difference between the three concurrent task conditions in this respect. In fact, the error data, if anything, suggested a *more* rapid improvement in performance with successive repetitions when there was high CE load. This trend was contrary to the prediction, as it 'started' with a higher rate of errors on switch trials in this condition, and decreased more rapidly with task repetition. The prediction that CE load would *not* affect task-specific preparation was, however, supported.

Analysis of the effects of previous trial interference

The analysis of previous trial stimulus valence effects was carried out in the same way as for Experiment 5a. A similar pattern of findings was expected, but if a high CE load impaired some inhibitory control mechanism responsible for the effects of interest, these were expected to be weaker, or absent, here in that condition. A repeated measures ANOVA with the following factors was carried out on median RT and on error proportions: Task switching (task switch/ task repeat), stimulus valence (univalent/ bivalent), valence on previous trial (univalent/ bivalent), and secondary task load (single task/ low CE load/ high CE load). Again, only effects involving the variable previous trial valence will be reported. The data are given in **Table 4.5**. Looking at the different load conditions separately, the RT data appear consistent, in the single task and low CE load conditions, with the predicted pattern for task repeat trials. Here a bivalent, as opposed to a univalent, previous trial was expected to be associated with faster RT on current bivalent trials, as in the previous studies. There was, however, some trend in the opposite direction in the high CE load condition, where it appeared that a previous bivalent trial impaired task repeat trial performance to some extent.

SINGLE TASK:		TASK SWITCH Previous univalent	TASK SWITCH Previous bivalent	TASK REPEAT Previous univalent	TASK REPEAT Previous bivalent
Univalent	RT in ms.	781 (202)	794 (220)	626 (106)	613 (104)
	% Errors	0.3 (1.5)	1.1 (3.4)	2.7 (4.6)	1.5 (2.5)
Bivalent	RT in ms.	906 (164)	917 (185)	745 (155)	720 (145)
	% Errors	7.8 (8.3)	7.9 (7.4)	3.3 (2.7)	3.7 (3.0)
LOW CE LOAD:					
Univalent	RT in ms.	796 (174)	819 (162)	642 (89)	653 (95)
	% Errors	2.2 (4.7)	2.2 (4.6)	0.7 (1.5)	1.8 (1.6)
Bivalent	RT in ms.	1004 (241)	984 (186)	850 (161)	789 (154)
	% Errors	5.6 (8.3)	9.8 (9.7)	4.3 (3.6)	4.1 (2.9)
HIGH CE LOAD:					
Univalent	RT in ms.	1056 (271)	1059 (267)	844 (261)	882 (312)
	% Errors	1.5 (4.1)	3.1 (4.6)	1.8 (3.2)	2.0 (2.4)
Bivalent	RT in ms.	1233 (359)	1230 (253)	1091 (293)	1107 (333)
	% Errors	9.0 (9.9)	11.7 (8.3))	6.7 (5.3)	7.4 (5.4)

Table 4.5. Experiment 5b: Sequential stimulus valence effects. Shows the effects of previous trial stimulus valence on performance, depending on current trial valence and on task switching. The different secondary task conditions are shown separately. Mean RTs and error proportions are given, with the standard error of the mean in brackets.

For response times, the overall ANOVA revealed no significant effects of previous trial valence. The planned analysis of RT for task repetition trials only showed a reliable interaction overall of previous and current trial valence ($F(1,23) = 3.93$, $p < 0.05$), and some trend towards a significant interaction of previous trial valence with secondary task load ($F(4,46) = 3.04$, $0.05 < p < 0.1$). Follow-up tests showed that the former interaction was due to a reliable decrease in RT on bivalent trials if the previous trial had also been bivalent ($F(1,23) = 4.68$, $p < 0.05$), but a non-significant increase on current univalent trials ($F(1,23) = 1.06$, n.s.). This was as predicted, and as found in Experiment 5a. However, the differences between pairs of load conditions, like the overall interaction, did not reach statistical significance.

The planned analysis for RT of task switch trials showed no significant effects of previous trial valence. This was as expected.

Error findings

Looking at error rates overall, these were higher where the previous trial had been bivalent (4.7%) than where it had been univalent (3.8%). This was reflected in a reliable main effect of previous trial valence ($F(1,23) = 7.47$, $p < 0.01$). There were also trends towards an interaction of previous stimulus valence with task switching ($F(1,23) = 4.12$, $0.05 < p < 0.1$), and towards a 4-way interaction ($F(2,46) = 2.65$, $0.05 < p < 0.1$).

As for RT, separate analyses of task switch and task repeat trials were then performed. This was done to allow comparison with the previous study, in which switch trial-specific effects of CE load had been found. For task repeat trials, there were no reliable effects of previous trial valence. For task switch trials, the error rate significantly higher with previous bivalent (6.0%) than with previous univalent trials (4.4%), as reflected in a significant simple main effect of previous valence ($F(1,23) = 6.66$, $p < 0.05$). The equivalent figures for task repeat trials were 3.4% and 3.2%, suggesting that task switch performance was chiefly responsible for the overall effect.

The data were also examined with respect to the cause of the trend towards a 4-way interaction. However, no reliable findings were found that supported the main theoretical distinction, between task switch and task repeat trials. There was some evidence of a trend mirroring the effect found in Experiment 5a, where previous bivalent trials were associated with an increase in errors on bivalent switch trials, in the secondary task conditions only (see **Table 4.5**). However, this was not statistically significant and so clearly cannot be said to have been *more* marked with implicit task cues.

Summary

The effects of previous trial valence on response times were not marked in this study, but the analysis of task repetition trials replicated the finding, in Experiment 4, that previous bivalent stimuli were associated with an improvement in performance on current bivalent trials. This was not found in the high CE load condition. However, there was no reliable difference between the latter and the other concurrent task conditions. There was also a trend overall towards poorer performance on current univalent task repeat trials if the previous trial was bivalent. This also provided some support for the findings of Experiments 4 and 5a. There was a general rise in error

rates with previous bivalent trials, which was shown to be due, almost exclusively, to task switch trial performance. Inspection of the data further suggested that this was mainly the result of data from bivalent trials in the two secondary task conditions. This had not been predicted, and its implications will be discussed below.

The prediction that CE load would reduce previous valence RT effects did not receive strong support, although there was some trend in this direction. Further evaluation of this prediction, which was for an effect of load with implicit task cueing only, awaits a formal comparison between the present study and Experiment 5a (see below).

Discussion of Experiment 5b

Task switching and repetition

The findings of Experiment 4 were broadly replicated in this study, but there were some apparent discrepancies. The effect of task switching/ repetition at long CTI was not reliable for RT, but was reliable for errors. This was accompanied by higher error rates in the experiment as a whole. In Chapter 2, the meaning of interference effects, reflected in the influence of stimulus valence on RT and errors, was discussed. Slowing of RT on both congruent and incongruent bivalent, compared with univalent trials, is taken to indicate interference at the level of the task set or rule, rather than competition between individual responses[4] (Monsell 1996). Errors on incongruent but not on congruent trials, on the other hand, are thought to imply that participants have responded according to the wrong task rule, either 'failing to switch' or failing to maintain the previous task set in some sense. The fact that the predicted valence effects on task switching and repetition appeared in the error but not in the RT data suggests that such 'wrong task' errors were less likely the more often the task repeated. Some contribution of increased response competition leading to errors is also possible, but does not affect interpretation of the present findings. In any case, task switching and repetition appeared to have a progressive effect on performance overall at long CTI, so this study cannot be said to differ significantly from Experiments 4 and 5a in this respect. There is no evidence to support the notion, raised in Chapter

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[4] As also mentioned earlier, this implies that the influence is on an effect, or subtracted quantity, such as switch cost or difference between successive task repetitions, so factors such as differences in stimulus encoding speed cancel each other out.

2, that implicit task cueing might be associated with greater 'residual switch costs', or switching/ repetition effects at long CTI.

As in Experiment 5a, neither secondary task had reliable effects on task switching and repetition. In the case of task-specific preparation, this was as predicted. For valence effects, however, it was not. The implications of this will be considered later, in the light of a more general comparison of the two studies.

Previous trial interference effects

The effects of the presence of interference from task-irrelevant stimulus information on the previous trial were as predicted for task repetition trials, in the experiment as a whole. However, they were not present in the high CE load condition, although it did not differ reliably in this respect from the rest of Experiment 5b. Although this finding fits with the prediction that such carry-over effects would be abolished by a high CE load, it is unfortunately difficult to interpret here because of the high error rate in that condition. On the assumption that the previous stimulus valence effect here represents carry-over inhibition of the competing task set or rule, perhaps not enough inhibition was taking place with an incongruent trial error rate of nearly 13%, since inhibition would require, at least, adopting the correct task set. On task switch trials, there was some evidence of a tendency for participants to make more mistakes when there had been a previous bivalent stimulus trial, in the two concurrent load conditions. This will be commented upon further in the General Discussion after a selective comparison of the two studies.

Comparison of Experiments 5a and 5b

Certain specific statistical comparisons between the present study and Experiment 5a were carried out, to address the predictions that have not yet been evaluated in full. Except where necessary, these look at RT data only, since this was the most important dependent variable.

Task switching and repetition

The first comparisons addressed predictions concerning progressive task switching and repetition effects with implicit and explicit task cueing. These were all tested by looking at certain pre-specified effects from a mixed model ANOVAs on median RTs.

The following factors were included: experiment group (explicit/ implicit task cueing), task switching/ repetition (switch trials, then 1st/ 2nd/ 3rd- 4th/ 5th- 8th task repetitions), cue-target interval (short/ long),), stimulus valence (univalent/ bivalent-congruent/ bivalent-incongruent), and concurrent task (single task/ low CE load/ high CE load).

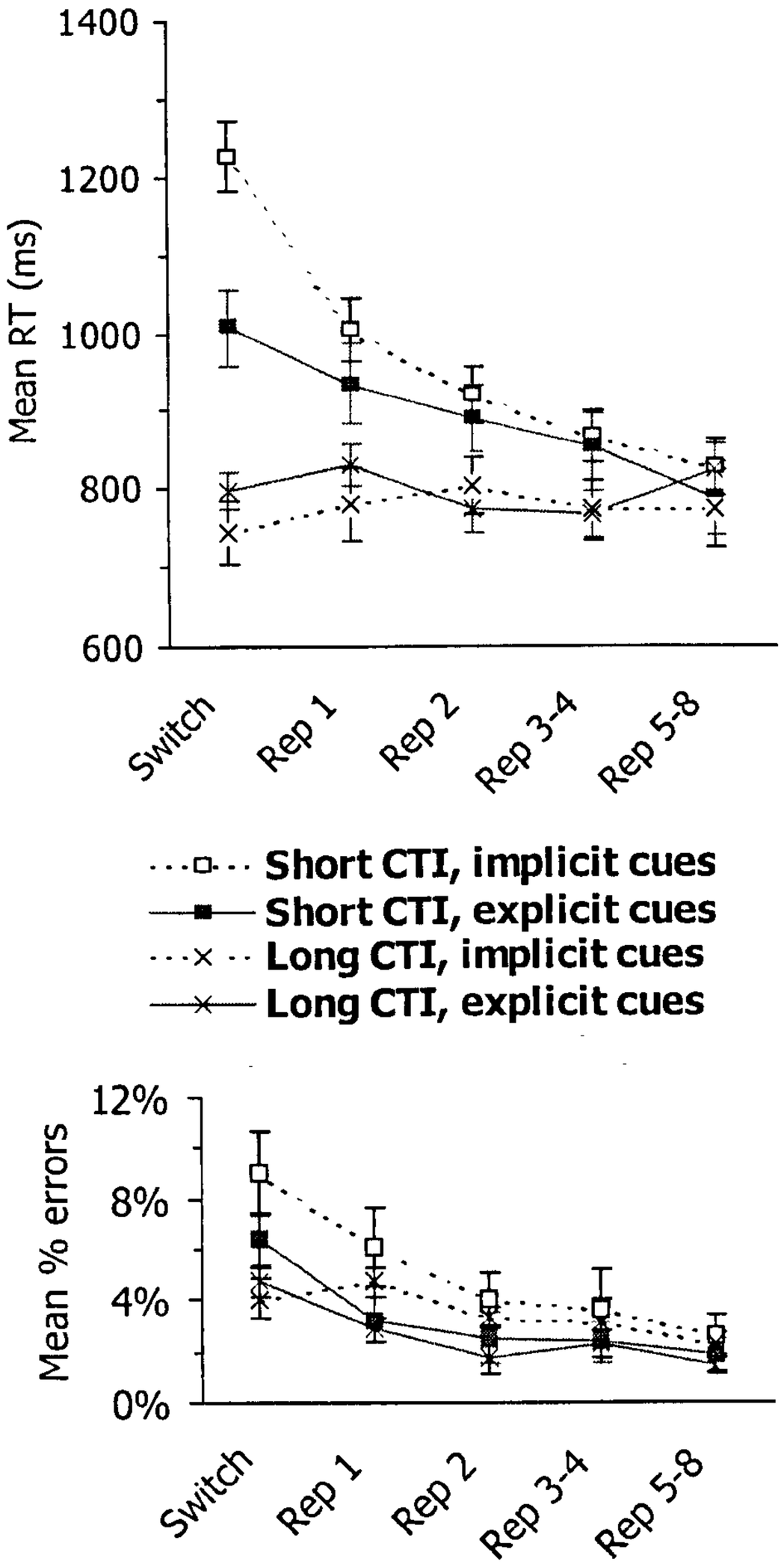

Figure 4.5. Effects of preparation on task switching and repetition in Experiments 5a (exlicit task cues) and 5b (implicit task cues). Error bars show the standard error of the mean.

It was predicted that task-specific preparation would be slower with implicit cueing, over several task repetitions. To test this, the 3-way interaction of experiment group, task switching/ repetition and CTI was examined, and this proved to be reliable ($F(4,184) = 13.41$, $p < 0.001$). Follow-up tests examined the simple interaction effect of group and task switching/ repetition at the two cue intervals separately. This was found to be reliable at short CTI ($F(4,184) = 13.07$, $p < 0.001$), with a clear interaction

between the decreasing linear trends in RT, with task repetition, for the two groups ($F(1,46) = 32.68$, $p < 0.001$) (see **Figure 4.5**). It was also reliable at long CTI ($F(4,184) = 3.32$, $p < 0.05$), again with a reliable interaction in the linear trends ($F(1,46) = 4.40$, $p < 0.05$). However, although there had been a reliable trend in the task repetition data at long CTI for Experiment 5a, there had been none such for Experiment 5b. In fact, if anything the trend there was if anything in the 'wrong' direction (see **Figure 4.3**). However, in Experiment 5b there was a reliable *error* repetition effect at long CTI, so this interaction may just have been the result of a lack of sensitivity to RT task repetition effects in that study. The most important reason for the overall interaction was therefore as expected, longer RTs in Experiment 5b than Experiment 5a at short CTI, reducing gradually with successive task repetitions.

Task cueing and interference effects

It was expected that the method of task cueing would *not* influence the effect of interference on baseline task switching/ repetition. Errors were also analysed because of the likely speed-accuracy trade-off, mentioned above. The finding that the interaction of experiment group with switching/ repetition and stimulus valence was not reliable for RT ($F(8,368) = 1.22$, n.s.), or for errors ($F < 1$, n.s.), was consistent with expectations.

Valence effects on RT were next examined independently of task repetition. Neither the interaction of experiment group and stimulus valence, nor the 3-way interaction of these factors with concurrent load, was reliable ($F < 1$, n.s. for both). However, both of these effects were significant for errors ($F(2,92) = 5.04$, $p < 1.01$ for 2-way, and $F(4,184) = 3.31$, $p < 0.05$ for 3-way). In the high CE load condition, participants made an average of 1.4%, 1.8% and 7.3% errors in Experiment 5a on univalent, congruent and incongruent trials, compared with 1.9%, 2.4% and 12.9% in Experiment 5b (see **Tables 4.1 and 4.4** for details of all load conditions in the two studies).

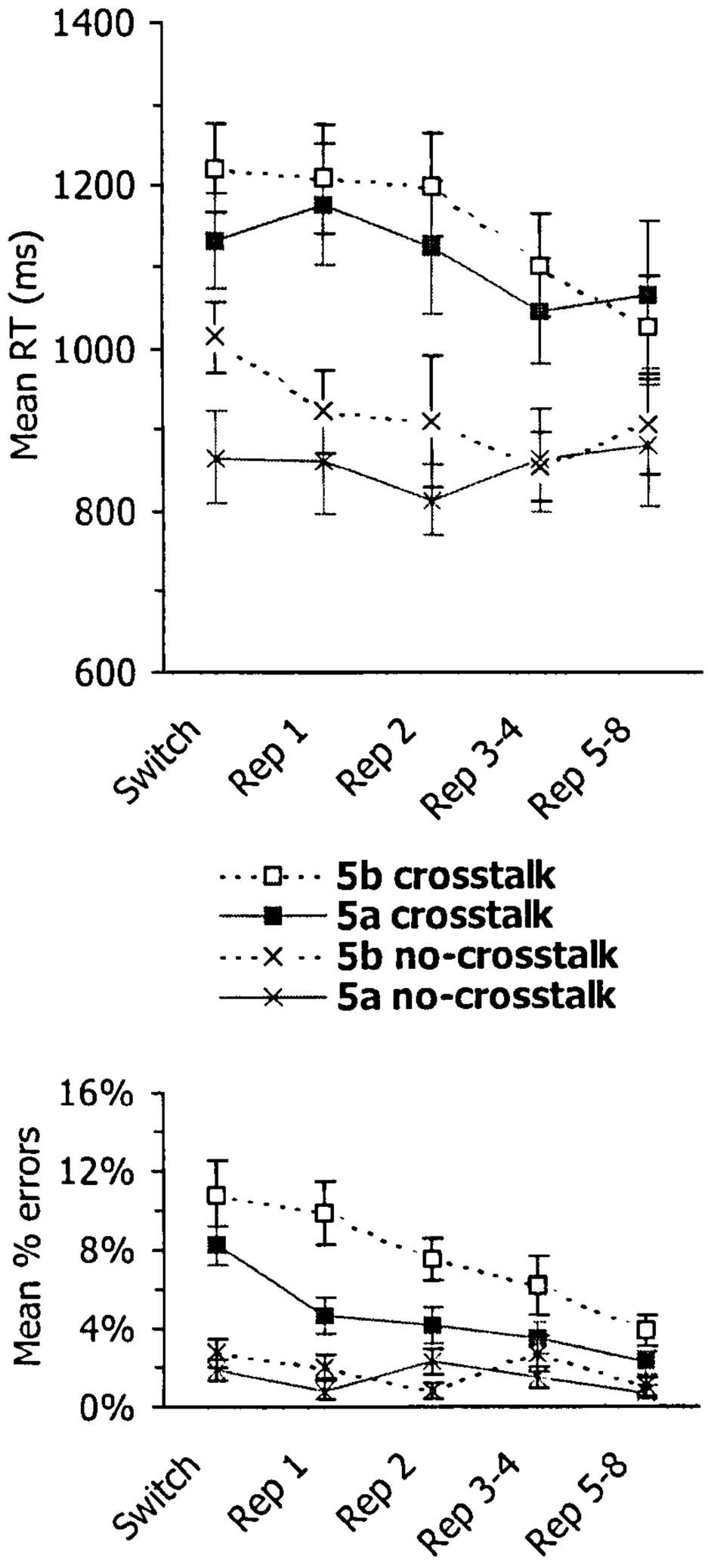

Figure 4.6. Effects of valence on task switching and repetition in the high CE load condition, for Experiments 5a (exlicit task cues) and 5b (implicit task cues). Error bars show the standard deviation of the mean.

Following up the 3-way interaction for errors, there was a reliable simple 3-way interaction effect for the comparison of the low and the high CE load conditions ($F(2,92) = 4.08$, $p < 0.05$), but not for the single task versus the low CE load conditions ($F < 1$, n.s.). The reason for the former difference was that there was a significant interaction between group and valence for the high load ($F(2,92) = 7.60$, $p < 0.005$), but not for the low load condition ($F < 1$, n.s.). In the high CE load condition, the two groups differed in error rates on incongruent trials ($T(46) = 2.95$, $p < 0.01$), but not on congruent ($T < 1$, n.s.) or univalent trials ($T(46) = 1.03$, n.s.). This was because participants made more errors in the high load condition with implicit

(Experiment 5b) than with explicit (Experiment 5a) task cueing.

With respect to the expected difference in the effects of interference at short and long CTI between the two studies, the interaction of experiment group with CTI and stimulus valence was clearly not significant ($F < 1$, n.s.). The prediction based on the pattern of findings in Experiments 2 and 3 was therefore not supported.

Finally, because a key prediction was that RT benefits from task switching/ repetition, on bivalent trials, would be reduced with high CE load, in Experiment 5b compared to 5a, the data were inspected in the high CE load condition for the two studies. It can be seen from **Figure 4.6** that, if anything, the trends for bivalent trials were towards a *steeper* decline in RT with task repetition in Experiment 5b than in Experiment 5a, contrary to predictions. These trends were also, if anything, more marked for the univalent trials. Thus the predicted effect of CE load and task cueing was clearly absent.

Carry-over effects of interference

Effects on task repetition trials

Since the only predicted sequential stimulus valence effects were on task repetition trials for RT, a comparison was made between the two groups using a mixed model ANOVA of median RTs on these trials only. This had the following factors: Experiment group (explicit/ implicit task cueing), stimulus valence (univalent/ bivalent), valence on previous trial (univalent/ bivalent), and secondary task load (single task/ low CE load/ high CE load). The effect of interest was the 4-way interaction of experiment group with concurrent load, stimulus valence, and previous valence, since sequential valence effects had been predicted to be impaired by high CE load, with implicit task cues only. However, this was not reliable ($F < 1$, n.s.). Minor qualitative differences between the data for the two studies, from this point of view, have already been commented upon.

General discussion

Both Experiments 5a and 5b broadly replicated the findings of Experiment 4, in that there were progressive performance benefits of several repetitions of the same task following a switch. The specific predictions with respect to task switching and repetition performance, and the sequential effects of stimulus valence, will be evaluated. Some theoretical implications will then be considered.

Task cueing and task-specific preparation

In both Experiment 5a and Experiment 5b the progressive performance benefit of task switching and repetition was reliably reduced by preparation following a task cue, being more marked at the short than at the long cue-target interval. This replicated the findings of Experiment 4. There were differences between the pattern of findings in the two studies, however, some of which were unexpected. The absence of the predicted task repetition effect at short CTI for univalent trials in Experiment 5a, and the repetition effects on errors in Experiment 5b, have already been discussed. More importantly, as predicted, there was also a marked difference between task repetition performance at short CTI with explicit and implicit task cues. In Chapter 2, the comparison between the same cueing methods in Experiments 2 and 3 showed greater switch costs at short CTI in Experiment 3 (implicit task cueing). The present two studies extended this finding over several task repetitions, to show that the effect of task cue type on task-specific preparation also applied when the task rule repeated, but became progressively smaller over several such repetitions. Regardless of the cueing method, task preparation was also unaffected by both secondary tasks. This was as predicted, consistent with the findings of Experiments 2 and 3 that neither low nor high concurrent CE load interfered with the effect of preparation on a task switch.

In Chapter 2 various possible explanations for an interaction between preparation for a switch and task cueing were considered. How may these be constrained by the extension of the findings over several task repetitions? It was considered likely that the additional processing involved in task-specific preparation with implicit task cues related either to retrieving the task rule, to activating it once retrieved, or to inhibiting the other rule. The present findings suggest that this processing contributes to response times when the task repeats as well as when it switches, but that this contribution decreases with every successive repeated application of the same task

rule. In other words, the 'extra' processing is faster, or has less work to do, with each successive task repetition.

There were two possible versions of the first, retrieval-based, explanation. Firstly, it was suggested that preparation for a switch was faster in Experiment 2 than in Experiment 3 because, with implicit cues, participants had first to recall which task was performed on the *previous* trial. However, there is no reason why the number of previous task repetitions should affect the latter process. The second, more parsimonious, possibility was a graded effect of cue *strength* on task rule retrieval. Retrieval might simply be slower with implicit cues, the difference being a quantitative one, with task-specific preparation influenced way by the *effective* strength of the task cue. This would be consistent with the interaction with task repetition, if previous repetitions increased effective cue strength and therefore the speed of retrieval. The third possibility, an explanation of the task cueing and preparation effect in terms of variations in inhibition of the currently-competing rule seems unlikely, as before, because there was no interaction of the effect with task-irrelevant interference. However, a gradual effect on activation of the correct rule, either increasing baseline rule activation with task repetition, or an increasing rate of activation, could account for the findings. These alternatives are not distinguishable without more data. For example, conducting a study with a range of cue-target intervals could determine whether the *rate* of task preparation is affected by cueing. A further empirical question would be determining whether effects on rule activation or retrieval could best account for the findings.

In Chapter 1, there was an interaction between the RT effects of task cueing, preparation and the control of interference *overall* (as opposed to interference with task switching). It is important to note here the negative finding that this was not apparent when Experiments 5a and 5b were compared, as it was suggested earlier that such a finding might provide some insight into cognitive control.

In Experiments 2 and 3 there was no evidence of involvement of auditory-verbal STM, or of the working memory central executive, in task preparation. The present two studies replicated this finding, and its implications have already been considered in Chapter 2. A more general consideration of the role of working memory in executive control is deferred until Chapter 6.

The task repetition effect in Experiments 5a and 5b was influenced, as expected, by task-irrelevant interference, which impaired performance to a progressively lesser extent with task repetition. In the former study, as in Experiment 4, this was evident in the RT data, the effect of task switching/ repetition in fact only being reliable for bivalent trials. In *both* studies, it was reflected in the error data (on incongruent-bivalent trials), but in Experiment 5b this was in the absence of an RT effect. However, this task repetition effect was not reliably different in the two studies, and therefore cannot be said to have been affected by task cue type, either for RT or for errors. There were more errors overall in Experiment 5b than 5a, and the majority were on incongruent-bivalent trials, suggesting that participants responded according to the wrong task rule more often with implicit than with explicit task cueing. However, such group differences in 'task errors' were *not* affected by how many times the same task was repeated. The overall appearance of difficulty with implicit task cueing therefore cannot explain the pattern of findings with respect to task repetition and stimulus valence.

Effects of CE load depending on cueing method

An important prediction for this study was that high CE load would impair the task repetition effect on bivalent trials only where task cueing was implicit. This was clearly not supported. The only difference between the two studies in terms of concurrent task effects was that in the high CE load condition, incongruent irrelevant stimulus attributes were associated with more errors in Experiment 5b than in Experiment 5a. This supports the idea that CE load impairs the suppression of irrelevant information. Although, unlike for the RT effect found in Experiments 2 and 3, it is not possible to be sure that this failure of inhibition occurs at the level of the task rule, rather than at the level of the individual response. It is consistent with an inhibitory role of the CE of working memory, as suggested by a number of investigators in other contexts (e.g. (Hasher, Rypma et al. 1989; Engle, Conway et al. 1995). However, it could also be that an overall load effect on error rates was not apparent because floor effects were operating on univalent and bivalent-congruent trials. A further possibility, and perhaps the most parsimonious, is that the incongruent trial errors resulted from a difficulty in remembering the correct task rule, when cueing is implicit. This was proposed as one possible explanation of the findings of Experiment 3. There is certainly nothing in the

present data to substantiate the claim that the effect of high CE load in Experiment 3, on task repeat trials, was due to a dependence of interference-related task repetition benefits on the central executive of working memory.

It is worth giving some further consideration at this point to the short-term memory based alternative to the 'executive inhibition' account of the RT slowing on bivalent task repetition trials in Experiment 3. It was suggested that participants' high error rates in the high load condition meant that they could have been guessing the task rule on about 40% of trials. This was thought probably to be because they forgot the task rule under a high CE load. Although in Experiment 5b, the task repetition effect was preserved, was there any evidence of a similar load effect here? This is related to the concern in the present study with enforcing performance accuracy. As explained earlier, this was not possible to the degree originally intended. However, a closer look at the error data from Experiments 2 and 3, and 5a and 5b, reveals that, despite this, some useful comment about the 'difficulty' of the implicit-cueing studies can be made. This comparison also has immediate relevance, in that it allows some tentative interpretation of the effects of high CE load under these conditions. **Table 4.6** gives a summary of the frequency of errors in Experiments 2 and 3, comparing them with the present studies.

EXPLICIT TASK CUES:						
Experiment 2		***Experiment 5a***				
Switch	**Repeat**	**Switch**	**1st repeat**	**2nd repeat**	**3rd- 4th repeats**	**5th- 8th repeats**
6.9%	3.7%	13.8%	8.2%	6.6%	4.1%	3.6%
IMPLICIT TASK CUES						
Experiment 3		***Experiment 5b***				
Switch	**Repeat**	**Switch**	**1st repeat**	**2nd repeat**	**3rd- 4th repeats**	**5th- 8th repeats**
21.8%	20.6%	18.4%	16.4%	13.2%	10.8%	5.3%

Table 4.6 Summary of error patterns in dual task experiments in Chapters 2 and 4, by task switching and repetition. Only incongruent trial error rates are given (as percentages).

The important issue here, focussing on Experiments 3 and 5b, is that the error rates on switch trials, probably indicating 'failures to switch' is similar, but the error rate on task repeat trials decreases progressively. However, these are still substantial particularly on the first task repetition. This is consistent with the possibility that under high CE load participants did not just respond to stronger task instructions, but found the

present study easier in terms of remembering the task rule specifically on later task repetitions. This fits with the idea that the central executive is involved in the on-line updating of information in working memory. If this is the case, then the 'executive' difficulty here lies not in task switching (as originally suggested), not in inhibiting competing task rules during task repetition (as proposed in Chapter 2), but in updating a record of what took place on the previous trial. On second and subsequent repetitions of the same task, a failure of such an updating process would be progressively less likely to give rise to a task-error, since if the information entered in working memory on the previous trial had been correct, then the correct task rule would be active.

The data do not fit the pattern expected if a control process failed to refresh or rehearse existing information in WM that was otherwise prone to decay. If that were the case, one would expect that only information about the most recent trial would be represented in WM. This might not lead to serious problems in the present experiments, since the use of the implicit cues relied only on one-trial-back information. If it did cause difficulties, they would not be affected by task switching and repetition, for the same reason.

An effect of high CE load here on updating information in WM is consistent with some views of control in working memory, in which there is a dissociation between the simple holding of information over a delay, and the holding of information with concurrent updating. This has been described as 'active maintenance', by O'Reilly and coworkers (O'Reilly, Braver et al. 1999). One of the main sources of evidence for such a dissociation is anatomical, where such complex WM tasks are found to activate the dorsolateral prefrontal cortex (DLPFC) in functional neuroimaging studies (e.g Jonides, Schumacher et al. 1997). It is not yet clear precisely what the determinants are of this complexity, however. In the 'n-back' task of Jonides et al, significant DLPFC activation is typically found only when the comparison of new material presented is to be made with remembered information from more than one trial back (2-back, and so on) (Jonides, Schumacher et al. 1997). In the present study, however, the comparing and updating of 1-back information may have had a greater demand than in the functional imaging studies. Here, here the switching task was performed at the same time, and, perhaps more crucially, the cues required translation essentially into 'same' or 'different' criteria before the comparison was made with the earlier trial to determine the new task. In the n-back task, on the other hand, the stimulus for comparison *is*

the token to-be-compared.

The case being made is for a particular working memory control mechanism, not a general central executive, although the data are consistent with the latter, broader, possibility. What is the significance here of the particular 'high load' WM task used? The use of serial mental arithmetic-based concurrent tasks in this thesis was supported by evidence, described in Chapter 2, that the CE is involved in producing approximately correct answers in serial addition and subtraction (Holding 1989; Logie, Gilhooly et al. 1994; Robbins, Anderson et al. 1996). The secondary task in these therefore had similar processing and maintenance requirements as the use of implicit task cues, and cannot therefore provide any additional evidence about the cross-domain character, or otherwise, of the processes involved.

Clearly these points require further empirical investigation. Some more general issues raised by this finding will be considered in Chapter 6.

Task cueing, load and sequential valence effects

No good evidence has been found that carry-over effects of previous trial interference on task switching and repetition are affected by the manipulation of task cueing used here, or by central executive load. The patterns of sequential stimulus valence effects on task repetition RT, in general, replicated the findings of Experiment 4. Most importantly from a theoretical point of view, previous bivalent stimuli were again associated with faster RTs to current bivalent stimuli. The high CE load condition in Experiment 5b was an exception to this, with a non-significant RT slowing being found, instead. The effect was not strong, and the comparison between the high load and the other two conditions in this respect was not reliable. Performance in this condition also did not differ reliably from that in the high CE load condition in Experiment 5a. However it is potentially important, as an abolition of the benefit of previous stimulus valence on task repetition trials was predicted in the high CE load condition, on the hypothesis that these carry-over effects depend on executive inhibitory control. Could this finding, as suggested earlier, have been secondary to the high error rate in Experiment 5b? This seems an ad hoc explanation, particularly as the error rate was fairly high in Experiment 5a (see **Table 4.6**). However, it can be put to the test by examining sequential valence effects on 2nd and subsequent task repetitions only, since the error rate was substantially lower there. Such an analysis was carried out, and although it did not reveal the predicted reliable RT speedup on current bivalent trials if

the previous trial was bivalent, it did show a trend in this direction, reversing the trend in the task repetition data as a whole.[5] This supports the idea that the loss of the carry-over effect in the high load condition was due to a high error rate, not to an impairment of executive control (directly).

A potentially interesting, but unexpected finding, is the consistent trend in Experiments 5a and 5b for previous stimulus valence to affect error rates, on task switch trials, in both the low and the high CE load conditions. The equal effects in both of these conditions (if anything stronger in the former) suggests a specific influence of verbal short-term memory on this sequential interference effect. With the concurrent tasks, participants were tending to respond according to the wrong task rule when the task switched, if the previous trial was bivalent. On a pre-switch bivalent trial, if the switched-from task set had to be imposed more strongly, this could produce such a pattern. Inhibition of the to-be-switched-to task would be expected to impair switching performance, but this would be regardless of the presence of irrelevant information on the switch trial. The data do not distinguish strongly between the two, since in Experiment 5b they were closer to the latter pattern. Replication is clearly desirable. However, some comments are possible now about what processes could have been responsible for the working memory effect, and these will be considered in the next section.

Theoretical implications for task control

Working memory

In terms of working memory, there is clearly no evidence here for an inhibitory role of the central executive in task switching and repetition. This does not rule out a role of CE inhibition in other circumstances (Hasher, Rypma et al. 1989; Engle, Conway et al. 1995), but constrains the situations where is likely to be important. The possibility has also been raised that the relationship to task repetition of participants' apparent difficulty in remembering which task to perform, when cueing is implicit and there is a high CE load, may be due to a role of the CE in the updating of information in STM.

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[5] The mean RTs on bivalent task repeat trials were 1082ms if the previous trial was univalent, and 1060ms with a previous bivalent trial, compared with 1091ms and 1197ms when the 1st task repetition was included.

What might underlie the unexpected effect of verbal STM load on error rates on switch trials where the pre-switch trial stimulus was bivalent? An obvious possibility is that verbal storage or rehearsal is involved, perhaps in the 'release' of inhibition of the old task, or activation of the new task. Such a contribution would be expected to involve the maintenance of verbal task rule information, and seems unlikely in the absence of any other effects of verbal STM load on task switching and repetition. Another possibility might be that retrieval of the new task rule from long-term memory is impaired by the concurrent retrieval, in both secondary tasks, of well-known number facts, although the details of such mechanisms are beyond the scope of this thesis. Interestingly in this context, in Rogers et al's study of task switching in patients with frontal lobe damage, the left frontal group not only showed increased switch costs, but also a pattern of performance on switch trials with previous trial interference similar to that in the present studies, although the impairment was in RT rather than in errors (Rogers, Sahakian et al. 1998). These issues will be considered further in the next Chapter, where a neuropsychological study of task switching and repetition in patients with frontal and posterior brain damage is presented.

In the list-method studies of Baddeley et al, articulatory suppression increased the task alternation cost, but only in the no-task-cues condition, where participants had to alternate, remembering which task to perform next (Baddeley, Chincotta et al. Submitted). Could the same processes explain this and the effects found here? This seems unlikely, since the secondary tasks did not affect switch costs in the present studies, and their influence on previous stimulus valence effects here was not dependent upon task cueing method. As has been noted before, different processes may contribute to 'mixing costs' (the difference between performing pure and alternating task blocks) and 'switch costs' (a difference between trials within one block) (see Los 1996). It also needs to be emphasised that not all manipulations of task cueing are the same (see Chapter 6 for further discussion of this).

Accounts of task control and switching

In the previous Chapter, the impact of the finding of progressive task repetition effects on theories intended to explain task switch costs was discussed in some detail. The present two studies have added to this the finding that the use of implicit, as opposed to explicit, task cues affects task-specific preparation in a graded fashion, over a number of successive task repetitions. This should constrain the cognitive models

further, in the sense that they must account not only for a task cueing effect on preparation for a switch, and for a graded task-preparation effect, but for the fact that the task-cueing effect is also gradual over several task repetitions. How do the various 'switching' models handle this new finding?

Monsell and coworkers' model cannot speak to the progressive task repetition effects, and so is not helpful in the present context (Rogers and Monsell 1995; Monsell, Azuma et al. 1998). Meiran's 3-component model regards most task-preparation as involving the reconfiguration of stimulus-sets, i.e. of selective attention to particular stimulus-attributes (Meiran 1999; Meiran In press). In terms of explaining the effect of cueing method on task-preparation, this is equivalent to the idea that selective non-spatial, like spatial attention, is affected by 'exogenous' versus 'endogenous' cueing. More general issues of cognitive control, such as this, will be returned to in Chapter 6. However, one point in support of this model from the present findings is the fact that the sequential effects of stimulus valence, and the effects of valence on task repetition, appear to be independent (see Discussion of Expt. 4). In Rubenstein et al's (in press) study, it was suggested that task-specific preparation consists of goal shifting in 'declarative working memory'. Task cueing effects were considered to be indicative of this type of processing, so the present finding cannot add much to this account, but it does need modifying to account for the gradual effect of task repetition, and the effect of cueing on repetition, over several trials (see also Discussion in the previous Chapter).

Conclusions

The two studies presented in this Chapter have examined the effect of concurrent working memory load on task switching and several successive task repetitions, with both implicit and explicit methods of task cueing. The findings of Experiment 4, that task repetition confers progressive performance benefits over up to eight trials, have been replicated. Task-preparation has again been shown to affect repetition in a graded fashion, and the slowing of preparation at the level of the task rule when implicit task cues are used, originally demonstrated in Experiments 2 and 3, has been replicated. Again, no evidence was found to support CE involvement in this. However, contrary to predictions, there was also no sign that the CE plays a role in generating interference-related task repetition benefits, and/ or sequential effects of irrelevant stimulus information on adjacent trials, via inhibitory control. Some theoretical

implications of these findings have been discussed, and the possibility raised that the use of implicit task cues does create some demand on the central executive, but in terms of updating information in working memory, rather than directly as a result of the need to switch, or repeat, tasks. Before considering the broader implications of these findings, however, a neuropsychological lesion study of task switching will be presented, in the next Chapter.

Chapter 5

A neuropsychological approach to task switching

Introduction

This study adopted a cognitive neuropsychological approach to understanding the control of task switching, taking as its starting point the claim that the ability to switch tasks depends upon the integrity of the frontal lobes (e.g. Milner 1963). The neuroanatomical correlates of executive processes involved in switching have not been considered so far in this thesis. However, the main purpose of this experiment was a cognitive one, to explore the evidence for specific control processes by studying a group of patients in whom deficits in such processes were expected. Using the task developed in the previous chapter, a comparison was made between switching performance in patients with focal frontal lobe damage, and those with posterior brain lesions, and healthy participants. Selective impairments of control processing were expected in the frontal patients. The present paradigm also allowed 'higher' control deficits, at the level of the task set or rule, to be distinguished to some extent from impairments in the control of individual responses.

In the investigations reported in the previous three chapters, two possible roles of executive control have been examined in the context of task switching. The first is task-specific preparation, which can affect a task switch or a task repeat. This has been described in a number of models as likely to involve 'executive' processing, but studies reported here have shown that it is not dependent upon the central executive of working memory, as indicated by the effects of concurrent working memory load. The second is the control of task-irrelevant information in order to respond according to the correct rule. In the task switching literature, this has generally been assumed to be automatic or 'exogenous' in nature. The experiments in the previous chapter indicated that the executive processes in working memory may play a role in this where there is a need for the active updating of task information. However, other concepts of executive control postulate a specific role for control in inhibiting prepotent action tendencies, for example performing the wrong task, or prepotent responses (e.g. Lhermitte 1983; Logan 1985; Shallice 1988). Deficits in patients in either of

these areas of control would help to further understanding of the processing involved in task control under normal circumstances. Associations between different measures reflecting the same putative control process would also be of interest, because these can support evidence from dissociations (Shallice 1988).

A secondary focus of interest was neuropsychological. As impairment can shed light on the structure of cognitive processing, so an understanding of cognitive processes can help explain patterns of impairment. Various attempts have been made to explain the bizarre behaviour often observed in patients with frontal lobe damage (e.g. Penfield and Evans 1935; Stuss, Gow et al. 1992) in terms of a number of underlying cognitive deficits. One of these has been the claim of mental inflexibility, or a difficulty in switching cognitive set (Milner 1964). This study offered an opportunity for a preliminary investigation of such a link between performance in the laboratory, and behaviour. Such links have proven difficult to establish in the past, but recent studies made the first step in this direction by attempting to correlate such 'dysexecutive' behaviour with specific cognitive deficits (Godefroy, Lhullier et al. 1996; Wilson, Alderman et al. 1996; Baddeley, Della Sala et al. 1997). Before introducing the specifics of this study, however, the evidence for frontal localisation of functions involved in task switching will be described. A highly selective review of the neuropsychological literature on executive control will then be presented, to place these processes in the context of wider accounts of frontal functioning.

Task switching and the frontal lobes

Switching in concept attainment tests

Milner studied the performance of a group of patients with focal frontal lobe damage on the Wisconsin Card Sorting Test (WCST Grant and Berg 1948; Milner 1963)). This is a complex test, in which participants are required to sort cards from a deck, one at a time. The cards show varying numbers of coloured symbols. Each card must be matched to one of four key cards that remain on the table, and for each a match is possible on all three dimensions of colour, form and number of symbols. Participants are not informed of the sorting principle during the test, but only told whether their response on each trial is 'right' or 'wrong'. The experimenter changes the relevant dimension every time 10 consecutive correct responses are made. The test is complete when 6 categories have thus been attained, or when all 128 cards have been

used. Milner found that patients with lesions of the dorsolateral prefrontal cortex (DLPFC) attained fewer categories and made more errors comprared both with patients with posterior lesions, and with a small group with temporal and inferior frontal lesions. The difference in error rates was due to a greater number of perseverative errors in the DLPFC group, due to cards being wrongly sorted according to the previously correct principle. This pattern of performance was attributed to an inability to overcome a previously established set, such as sorting cards by colour. This pattern of performance has since become known as being 'stuck in set' (Sandson and Albert 1984).

The generalisability of this finding has, however, been challenged. There is good evidence that perseverative errors on the WCST are not specific to patients with frontal damage. They are also not found in all patients with DLPFC, or other frontal, lesions (for review see Reitan and Wolfson 1994). Recent research has therefore attempted to tease out some of the processes involved in failures on the WCST. It has been pointed out by Burgess and Shallice (1996) that a tendency to apply bizarre hypotheses would also be reflected in apparent 'perseverative' errors on the WCST, because of the design of the test. In the latter study, this was explored by using a novel concept attainment task, the Brixton test (Burgess and Shallice 1996). Results showed that patients with anterior cerebral lesions did indeed make more bizarre responses than those with posterior lesions. Rates of perseveration of the previous rule were similar in the two groups.

Owen and coworkers have also carried out studies of rule acquisition and shifting, using a computerised test. As in the WCST, participants learned novel rules using experimenter feedback, and unpredictable rule shifts occurred. Rules again changed once a criterion has been reached. Frontal patients were initially found to make more errors than age-matched controls after a rule change, but only when the shift was 'extradimensional' (e.g. responding to a particular type of shape, after having responded to a particular type of line) as opposed to 'intradimensional (e.g. responding to a novel type of line) (Owen, Roberts et al. 1991). Patients with posterior lesions did not make more errors than their controls; however, they were slower. A later study showed that patients with frontal lobe damage were selectively impaired at switching *away from* the old rule, rather than switching *to* a new rule, whereas the converse was true of medicated patients with Parkinson's Disease (PD; although non-medicated PD patients showed both impairments) (Owen, Roberts et al. 1993). Again, each patient

group was compared with age-matched healthy controls.

The latter finding suggested that frontal lobe impairments on the WCST may indeed be due to problems in switching away from a previously performed task. However, in the WCST and in Owen et al's task, participants are required to detect and to learn new rules from experimenter feedback, as well as shifting from rule to rule. In Burgess and Shallice's (1996) study, on the other hand, it is possible that the arbitrary and unfamiliar rules used in the Brixton test reduced any tendency to be 'stuck in set'.

Neuropsychological studies of switch costs

Some recent neuropsychological investigations of task switching have used a different approach, applying paradigms developed in studies of switching in the healthy population. Rogers et al compared the performance of patients with frontal lobe damage and age-matched healthy controls on a task involving predictable alternation between number and letter classification (Rogers and Monsell 1995). The strength of task cueing was manipulated, and was either 'weak' (the task on each trial being cued by colour change as well as by sequence), or 'strong' (where a cue word stated the name of the task). Switch costs were greater in the left frontal patients than in the right frontal patients and controls, but this was only the case in blocks where some stimuli had congruent or incongruent irrelevant characters[6]. Within these blocks, performance did not differ on bivalent or univalent *trials*. Switch costs overall were reduced with strong task cueing, but there was no evidence that this was affected by frontal lobe damage. Task repeat trial performance was not reliably affected by lesion group.

Rogers et al also examined the influence, on task switch trials, of irrelevant stimulus attributes on the previous trial. Congruent or incongruent, as opposed to neutral, previous trials were associated with a slowing of RT on switch trials in the left frontal group, but only where this was also bivalent. Where the switch trial was neutral, a previous congruent or incongruent trial was associated with RT facilitation in the left frontal patients. In both of these cases, left frontal lesion patients differed from the right frontal group and the controls. The findings were attributed to a stronger negative priming-like effect in the former group. The present study offered an

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[6] All stimuli had irrelevant characters, but some were neutral, as in Rogers and Monsell's (1995) study.

opportunity to replicate this finding, and to see whether such an effect is consistently associated with a deficit in task switching. It should be noted that in Rogers et al's study the response-stimulus interval (RSI) was 1000ms, so performance would be expected to resemble that at long CTI in the present paradigm (Rogers, Sahakian et al. 1998).

Rubenstein et al have reported data from single-case and group studies of task switching in patients with frontal lobe damage (Meyer, Evans et al. 1998; Rubenstein, Meyer et al. In press). In the former, patient T.S., who had a left dorsolateral prefrontal lesion, was found to have increased switch costs on a geometric-object classification task. This increase was more marked where there was interference between tasks from irrelevant stimulus attributes. These findings were interpreted as showing, at least, a deficit in the rule activation executive control process of the authors' model of switching, which has already been described in some detail in this thesis.

Neuroimaging data

Neuroimaging studies have recently attempted to localise processes specific to task switching. In an experiment using positron emission tomography (PET), Meyer et al demonstrated increased activation with task alternation in the left dorsolateral prefrontal cortex and anterior cingulate, compared with a pure task condition (Meyer, Evans et al. 1998). The tasks were the same as used in the study with patient T.S, and the findings therefore supported those of the lesion study. An electrophysiological study has also been carried out, in which where participants switched between different spatial classification tasks using opposite hands. Switch trials were associated with ERP events in both occipital and parietal lobes, but also the frontocentral area (Moulden, Picton et al. 1998).

The evidence for a localisation in the frontal lobes of important functions for task switching is therefore mixed. The lesion studies, as well as Meyer et al's PET experiment, do provide some evidence linking task switching with the left frontal lobe, possibly the dorsolateral area specifically (although it is not possible to rule out the influence of task-specific factors on laterality effects). However, the frontal lobes are clearly not involved in task switching under all circumstances. The presence of interference between tasks appears to be an important factor, but others remain to be determined. The current study was designed before results from the work of Rogers

et al (1998) and Rubenstein et al (in press) were available, but aimed to examine a group of patients likely to show switching deficits in order better to understand the processes normally involved in task switching. Before describing this experiment in detail, it is necessary to consider briefly the claims made for a wider role of this part of the brain in control processing.

Executive control and frontal lobe function

If the frontal lobes play a role, under some conditions, in switching between tasks, what processes might underly this? Are they 'local' control processes specific to switching, consistent with the model put forward by Rubenstein et al (in press)? Or are they 'global' or executive processes that have broader roles in cognitive control? Clinical observation of patients with frontal lobe lesions has long given the impression that, while they are generally not impaired on the basic skills needed to perform the components of a complex task, they often have difficulty in organising and controlling task performance as a coherent whole (e.g. Penfield and Evans 1935). It was such observations that sparked initial interest in the frontal lobes as the seat of 'higher', 'executive', or control processes (Teuber 1964; Luria 1966). A number of cognitive models have since made this explicit (Fuster 1980; Norman and Shallice 1980; Duncan 1986; Stuss and Benson 1986). It is claimed that the supervisory attentional system (SAS) described in Norman and Shallice's (1980) model of action control is localised in the frontal lobes. That region of the brain is therefore said, after Luria, to be responsible for the programming, regulation and verification of activity (Luria 1973; Shallice 1988).

A selective account will now be given of the evidence for a frontal lobe contribution to the two broad areas of cognitive control that have so far been discussed in this thesis. These are goal-directed processing, and the overcoming of interference. As described earlier, these have been linked to the Norman and Shallice model in the context of task switching (e.g. Rogers and Monsell 1995). The present study did not attempt to test such broad notions directly, but assumed that the relationship of the frontal lobes to particular putative executive processes, such as those involved in task switching, is an empirical matter.

The frontal lobes, cueing and goal-directed behaviour

There is a variety of evidence which suggests that the frontal lobes are involved in the

control of goal-directed processing. It is well documented that patients with damage to the frontal lobes may have difficulty in following task instructions (e.g. Milner 1964; Luria 1973; Duncan 1986; Shallice 1988). They are typically able to verbalise them, but do not act accordingly. Duncan as described this behaviour as 'goal neglect' (Duncan, Emslie et al. 1996). There are a number of potential processing deficits behind this, and the current study examined one of these, the use of advance task information. The importance of the maintenance in working memory of goal-representations is noted, as is the relevance of the strength or quality of environmental cueing, but the present study did not examine these directly.

The question here is whether part of frontal lobe patients' behaviour derives from a difficulty using external cues to control action. Milner suggested that the reason this group perseverated on the WCST was because they did not make use of the varying environmental signals (feedback from the experimenter) (Milner 1964). The neuropsychological studies of switching described above did not manipulate preparatory interval. Little is known about the role of the frontal lobes in goal-directed preparatory activity, but, logically, it need not be dependent upon cue strength. Alivisatos and Milner showed that a group of frontal patients were impaired in the use of advance information in a spatial choice reaction task, in comparison to posterior lesion and healthy controls (Alivisatos and Milner 1989). In the present study, as elsewhere in this thesis, non-spatial attention switching was of interest, bearing in mind that it is not known to what extent the processing may overlap with spatial shifts of attention (see Chapter 1). 'Strong' task cues were used, so the need for goal-directed preparation would not be confounded with the requirement for monitoring or endogenous control. Such distinctions between component processes are not possible in the WCST or Owen et al's (1991) shifting paradigm. Secondly, switching tasks and changing or repeating responses are separated, unlike in Moulden et al's (1998) study, so switching deficits cannot be accounted for by failures to control individual responses.

The frontal lobes and the control of interference

The second relevant area of task control where patients with frontal lobe damage have been said to show impairments is the control of task-irrelevant interference (Luria 1966; Lhermitte 1983). This is partly based on clinical observations of distractibility (Rylander 1939), but there is also experimental evidence of impaired ability to

concentrate in patients with frontal lobe (e.g. Knight, Hillyard et al. 1981; Bianchi, Zolo et al. 1993; see Foster, Eskes et al. 1994 for review; Godefroy, Lhullier et al. 1996). Deficits in Strooplike tasks have been found in some studies (Perret 1974; Vendrell, Junque et al. 1995), but not in others (Stuss, Benson et al. 1981). Several functional imaging studies have shown frontal lobe activations in Stroop tasks (e.g. Pardo, Pardo et al. 1990; Bench, Frith et al. 1993). According to the Norman and Shallice (1980) model, task-level interference occurs where there are environmental triggers to existing but currently inappropriate action schemata. It is assumed that under normal circumstances the SAS, damaged in these patients, would inhibit the inappropriate action plan (Shallice 1988). Thus frontal patients' inappropriate behaviour is said to be due to a failure of inhibition at a high level, that of an task schema, set or rule. This fits with the idea that frontal damage leads to 'stuck in set' perseveration (Milner 1963; Sandson and Albert 1984; Owen, Roberts et al. 1993).

However, in order to establish that there is a complex, higher level cognitive deficit it is necessary to distinguish it from other potentially simpler explanations, such as failure to inhibit an particular response? Although difficulty in response control could not explain deficits on the WCST and other concept attainment tasks, since responses and task rules are manipulated independently, they could account for claims that impaired performance on other tests results from being stuck-in-set. Examples of this are the object alternation and delayed alternation tasks, which cannot distinguish these two potential levels of control (Freedman, Black et al. 1998). This is not just a theoretical possibility, since there is ample evidence from both case reports (e.g. Luria 1965) and group studies that patients with frontal lobe lesions may show deficits in response control. They have been shown to make errors of commission in go - no go tasks, as well as in other situations where the S-R mapping remains constant (Drewe 1975; Decary and Richer 1995; Godefroy, Lhullier et al. 1996).

Deficits in task switching may also be secondary to broader impairments in task control that can affect switching. Two factors in this may be working memory impairment, or goal neglect, as indicated above. More importantly in the present context, it should be noted that increased switch costs in patients with frontal damage have so far only been found where there has been interference between tasks (Rogers and Monsell 1995; Rubenstein, Meyer et al. In press). This apparent impairment in switching could therefore be part of a more general deficit in the control of interference. The present paradigm allows this distinction, too, to be made.

Methodological issues

This experiment employed a mixture of association and dissociation methodology in a neuropsychological group study. Although drawing inferences from associations has for some time been controversial in cognitive neuropsychology, it is now recognised that the study of executive function demands a slightly different approach (see Shallice 1988 for detailed discussion; Burgess 1999). The main reason for this is that most cognitive models, and certainly that of Norman and Shallice (1980), propose a hierarchical relationship between executive and more automatic processing. The 'gold standard' of a double dissociation as a means to identify separate mental processes cannot be obtained between executive and non-executive processes, as the former cannot be measured without the latter also being involved in task performance (Shallice 1988).

Conversely, if the same control process is hypothesised to be involved in two different domains (for example spatial and verbal), some correlation is expected between performance in the domains, although superficial task demands differ. Such associations will generally not be large in magnitude, but are potentially as important as dissociations for understanding executive control. In the context of this study, associations were predicted between standardised cognitive tests that are thought to include a particular executive component, and measures from the experimental RT task, designed to tap the same constructs.

A group study approach was chosen because this is necessary in order to obtain statistically reliable information about associations in performance (Burgess 1999). It was also preferred because variations in strategy and individual experience are likely particularly to affect the study of executive processing, because what is novel and requires such processing for one person may be routine for another. Some averaging across individuals is therefore desirable if inferences about the cognitive architecture of the majority are to be made.

By the definition used here, domain-specific control processes are not executive. The extent to which executive processing, if it exists in this sense, can be said to be fractionable into clear subcomponents, remains a relatively open question. One view regards executive processing (by the frontal lobes) as the likely cause of the reliable correlations of individuals' performance across a range of tasks, known as the 'g' or general factor (Spearman 1927; Duncan, Burgess et al. 1995). The existence of only

one such factor does not necessarily imply a single process of 'intelligence' but does suggest that the processes reflected in 'g' act together and may not be dissociable in the traditional sense (see Duncan, Emslie et al. 1996). At the same time, however, there are claims that the whole 'executive' can be fractionated into distinct subprocesses (e.g. Burgess and Shallice 1994; Baddeley 1996).

Finally, it should be noted that like many cognitive (as opposed to clinical) neuropsychological studies, this study was not set up to make inferences about frontal lobe patients in general, and no attempt was made to obtain a representative sample of this population. In a similar fashion, its main aim in comparing patients with anterior and posterior lesions was to obtain functional information as far as possible unconfounded with any general effects of illness and brain damage. A number of studies have successfully employed this method, demonstrating task control deficits only in the frontal group (e.g. Milner 1963; Bianchi, Zolo et al. 1993; Decary and Richer 1995; Burgess and Shallice 1996; Godefroy, Lhullier et al. 1996; Richer and Lepage 1996).

Specific aims and predictions of this study

The evidence presented in the previous two chapters has suggested that, in the present speeded response paradigm, control of performance at the level of the task set or rule operates on trials where the task repeats as well as trials where the task switches. Both were therefore examined in the present lesion study. In the previous two chapters, two factors affected performance of a task switch and a task repetition. The first was cue-driven preparation for a specific task, and the second was interference arising from task-irrelevant information in the stimulus. The predictions, given below, differ from those for Experiments 2, 3, 5a and 5b, being based on putative roles of the frontal lobes (as opposed to the central executive) in task switching. They fall into three categories, the first of which concerned the performance expected from patients on standardised neuropsychological tests of executive function. The second was assessed by Experiment 6a, and described patients' predicted performance on the RT test of task switching, in the areas of goal-directed behaviour and the control of interference. The third set of predictions focussed on the control of interference, contrasting the levels of the task rule and of the individual response. These were examined in Experiment 6b, using a pencil and paper test of perseveration.

Associations between switching and standardised tests

Standardised neuropsychological tests of executive function were administered to patients in this study to enable comparison with performance on the experimental measures of executive function. Particularly important was the Wisconsin Card Sort Test WCST, in the sense that failure on this has been the commonest basis for claims that frontal damage leads to a difficulty switching task set. A secondary test of switching was the Trails test, in which participants join up sequences of numbers or numbers and letters on a page. A difficulty shifting between two tasks is said to be reflected in part B, where alternation between numbers and letters is required (Lezak 1983). Other functions tested were susceptibility to inter-task interference in a Stroop test (Trenerry, Crosson et al. 1989; see Lezak 1995), and verbal fluency with generation of words beginning with particular letters (Benton 1968). The latter ability may depend on the strategic retrieval of items from memory, and on switching between retrieval strategies (Troyer, Moscovitch et al. 1998). This study was not concerned directly with working memory, although forwards and backwards digit span was measured as part of the patient screening procedure.

In general, significant correlations in performance on specific standardised tests and the experimental measures would be taken as evidence that the same executive processes were involved in both. This was because the non-executive demands of the tasks were assumed to be different (see discussion of methodology, above). The details of test scoring are described in the Methods and Results sections, as appropriate.

Perseverative responding on the WCST (Heaton 1981) was predicted to correlate negatively with the RT gains from task switching and repetition, at least in the frontal lobe group. The same was predicted for poor performance on the Trail Making Test, part B only. The assumption was that if participants were stuck-in-set, their performance would show little improvement after a switch. Standardised Stroop test performance was predicted to correlate with performance decrements, either in RT or errors, due to task-interference in the experimental task (independent of task switching/ repetition). Performance on the FAS word fluency test was not, on the other hand, expected to correlate with any measure of task switching from the experimental test.

Measurement of dysexecutive behaviour

The method used here to assess dysexecutive behaviour was the 'DEX' dysexecutive behaviour questionnaire, developed as part of the Behavioural Assessment of the Dysexecutive Syndrome (BADS Wilson, Alderman et al. 1996) test battery. This was based on Stuss and Benson's classification of behavioural effects of frontal lobe damage (Stuss and Benson 1984). It aimed to produce an assessment of dysexecutive behaviour that was specific and not just sensitive, and is more than a measure of neurological damage (Burgess, Alderman et al. 1998). In previous studies that have linked cognitive deficits to such behaviour, assessment of the latter has been in the form of the opinion of experienced clinicians (Godefroy, Lhullier et al. 1996; Baddeley, Della Sala et al. 1997). The DEX attempts to achieve a similar aim, consisting of 20 questions in two separate questionnaires, one given to the patient and one to a relative or carer who knows their everyday behaviour well (see copy of the questionnaire in **Appendix B**). Wilson et al (1996) found that the most useful measure was the difference between the *independent-* and *self-* ratings. This correlated significantly with perseveration on the Modified WCST (Nelson 1978), with alternation performance on the Trails test (for details see Method), and with verbal fluency, particularly the 'FAS' test (see Method), but not with WAIS IQ or NART. The independent-rater score also predicted executive test performance to some extent. The results therefore supported the traditional impression that dysexecutive patients tend not to be aware of their deficits (e.g. Stuss and Benson 1984) so assessment is best made by an observer. In this study, it was expected that the frontal lesion group would show greater dysexecutive behaviour scores than the posterior lesion group or the controls. If this were the case, the aim was to see to what extent this was associated with deficits in task switching.

Experiment 6a: Task switching with speeded responses

Switching and goal-directed behaviour

The earlier discussion of the neuropsychological literature has indicated that frontal lobe damage may lead to difficulty in following specific goals. One underlying cause of this may be the neglect of external cues needed for task performance. Part of the predicted pattern of performance related to residual switch costs. De Jong (1995) has claimed that these can be explained by 'unpreparedness' for a switch, because people do not always act effectively on task cues or expectations. Although no firm evidence

of residual costs has been shown in healthy participants in the studies reported so far in this thesis, it was possible that frontal patients would show such a pattern because of goal neglect. This experiment therefore examined one possible mechanism behind goal neglect in frontal patients that is also involved in task switching, and did not attempt to explore the concept of external cue *strength* (see e.g. Rogers and Monsell 1995).

The basic prediction was therefore that patients with frontal damage would be impaired at task-specific preparation during a long cue-target interval (CTI), because of a failure to make use of external cues. The effect of CTI on task switching/ repetition performance would therefore be absent or reduced compared to patients with posterior lesions, or controls. As a result, a larger task switching/ repetition effect was predicted at long CTI in the frontal lesion group, since it was expected that there would be more task-specific processing still to be carried out after a long preparatory interval.

Set switching and being 'stuck-in-set'

The present study aimed, where possible, to dissociate processes involved in control at the level of a task set or rule, from 'lower level' control operating at the level of specific responses. The first question was whether frontal patients have a general difficulty with flexibility at the level of the task set or rule, that is an overall impairment in switching set, not necessarily related to the presence or absence of goal neglect. Other switching studies, and in particular the lesion studies of Rogers et al (1998) and of Rubenstein et al (in press), have used 'switch costs' as the dependent variable. However, in the light of the findings of Experiment 4, it was thought preferable to consider benefit from more than one task repetition.

In the frontal lesion group, less of an improvement in overall RT was predicted after a task switch trial, over successive task repetitions. The tendency to become stuck in set was expected to be greater where stimuli are ambiguous and associated with more than one task, so such an deficit might only be found for bivalent stimuli, whether reflected in RT or errors ('failures to switch'). This was predicted in the absence of preparation, i.e. at short CTI.

Inhibitory control of competing task information

Further predictions were made about possible deficits in controlling interference from the non-current task, not necessarily related to task switching and repetition. Patients with frontal lobe lesions were predicted to be more affected by task-interference where stimuli were bivalent, over all levels of task switching/ repetition. Deficits of inhibitory control at this level might also be reflected in the effects of previous trial stimulus valence. When the task switched, RT slowing on bivalent trials was predicted where the previous trial had also been bivalent, in the frontal lesion group only, thus replicating the findings of Rogers et al (1998).

Inhibitory control at the level of individual responses

The RT switching task and also allowed some assessment of deficits in response control. In choice reaction time tasks, RTs are generally faster where the response is repeated than where the response is different from that on the previous trial (Bertelson 1961). It appears not to be repetition of the motor response per se that is important, however, but repetition of the stimulus classification, clearly a higher level factor (Pashler and Baylis 1991; Campbell and Proctor 1993). However, the effects of response repetition have consistently been found to be different when the task rule switches, with the apparently paradoxical finding that repetition of the response leads to an RT cost rather than the expected benefit (Rogers and Monsell 1995; Meiran 1996). Meiran has suggested that this occurs because the to-be-repeated response code (such as 'left') is still linked to the switched-from task rule (Meiran 1999).

However, a pathological tendency to repeat previous responses could also influence the pattern of findings in patients with frontal lobe damage. On task switch trials, the prediction was of a trend in the opposite direction to that found in healthy participants. Despite the usual paradoxical RT slowing and error increase with response repetition, if frontal lobe damage leads to a general tendency to repeat the same response, more inappropriate repetitions would be predicted on switch trials too. With a concomitant RT effect, frontals would be expected to show *less* of a paradoxical slowing with response repetition than posteriors and controls.

On task repetition trials, a selective increase in inappropriate response repetitions was expected in the frontal lesion group. Such an effect would be in same direction as that found in healthy participants, and a prominence of errors would be notable, as the latter group make very few errors on task repetition trials under single task conditions (see earlier experiments).

If the expected deficits were found on any or all of these areas of executive control, the aim was to determine to what extent these predicted dysexecutive behaviour, as assessed by the DEX questionnaire.

Experiment 6b: Perseveration of task set and response

This part of the study was based on Sandson and Albert's (1984) stuck-in-set test. This brief pencil-and paper examination is able potentially to distinguish between a tendency to be 'stuck' in a previous task rule and a tendency towards perseveration of individual responses. It is based on the repeated copying of two shapes followed by the reversal of the S-R mappings, i.e. a requirement to draw the shape *not* shown in the stimulus. Before the copying condition, there was a first 'establishing task set' part. This differed from the original, since Sandson and Albert required participants to draw the stimuli in response to words. The idea here was to establish a 'set' of classifying the stimuli in terms of their shape, since this would remain constant throughout the test, rather than mapping a classification to a specific response, since these mappings would vary during the test in any case.

'Stuck-in-set' perseveration was predicted in the frontal patient group. This would be reflected in a selective slowing of performance and increase in errors when the S-R mappings were reversed. Perseverations at the level of the response, also predicted in the frontal group, would be reflected in a preponderance of inappropiate response repetitions (as opposed to inappropriate alternations) in both copying and reversal conditions. Although deficits at both levels of control were predicted to result from frontal lobe damage, these two aspects of performance were not expected to correlate with each other.

Method

Participants

Patients were recruited from hospitals in the Frenchay and United Bristol Healthcare Trusts. Permission was obtained for the study from both trusts' research ethics committees, and from the consultant physicians and neurosurgeons responsible for the patients' care.

General recruitment criteria

Age limits were set for the study, with a maximum of 80 years, and also a minimum of 30 years. The latter was to avoid including patients with low-grade tumours that had originated before their brain had developed fully[7]. Any aetiology of localised brain damage was acceptable, but frontal and posterior groups were matched as far as possible from this point of view. Onset of the brain damage, or surgery (in the case of tumours) had to have been at least 6 weeks prior to testing. Because of the anatomical inclusion criteria, all patients had either had a CT or MRI brain scan since the relevant event, or had a neurological deficit that unambiguously localised the lesion to an appropriate area (one posterior patient; see below). Patients were excluded from the study if they had other localised or generalised medical conditions that were likely to affect the brain, or if they were taking medication known to cause cognitive impairment. Anticonvulsant drugs were an exception to the latter condition, and were taken by five patients in the frontal and nine in the posterior group. Individuals with a history of psychotic illness were also excluded, as were those whose scans showed evidence of other cerebral disease, even if there was no apparent cause.

Relevant clinical details of both groups of participants with brain damage are given in **Table 5.1a and b.** Time of onset is given for general information, but a quantitative comparison between groups would be difficult to interpret in view of the different pathologies.

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[7] Thanks to consultant pathologist Dr. Seth Love at Frenchay Hospital for this advice.

Frontal and posterior lesion groups

No.	Age	Aetiology of brain damage	Time since onset	Side	Site of lesion	YOE	NART FSIQ
F1	66	Vascular (subarachnoid haemorrhage)	2 mths	Bil	Orbitomedial bilateral	9	102
F2	57	Vascular (cerebral haemorrhage)	2 mths	R	Posterolateral small	19	128
F3	50	Meningioma, surgery	5 mths	L	High polar/lateral, small	11	112
F4	64	Meningioma, surgery	4 mths	L	Inferior, small	9	110
F5	52	Excision of epileptic focus	66 mths	L	Polar	11	102
F6	75	Vascular (cerebral infarct)	4 mths	L	Deep lateral white matter, small	9	108
F7	35	Vascular (cerebral haemorrhage & surgery)	30 mths	R	Polar, small	11	123
F8	67	Low grade intrinsic tumour	30 mths	L	Including middle F gyrus, to superior F gyrus near vertex	19	128
F9	59	Cerebral abscess & surgery	30 mths	R	Posterior upper frontal convexity	14	120
F10	50	Low grade intrinsic tumour, surgery	30 mths	R	Lateral cortical, large	13	123
F11	49	Meningioma, surgery	15 mths	L	Lateral cortical, small	14	116
F12	44	Low grade intrinsic tumour, surgery	14 mths	R	Mostly white matter, extensive infiltrating	10	95
F13	49	Low grade intrinsic tumour, surgery	7 mths	R	Including anterior cingulate & corpus callosum, extensive infiltrating	16	124
F14	35	High grade intrinsic tumour, surgery	6 mths	L	Posterolateral	11	81

Table 5.1a. Clinical details of frontal lesion group. Included here is years of full-time education (Y.O.E.), and the estimate of premorbid Full Scale IQ based on scores on the National Adult Reading Test (NART). Time of onset is given as defined in the text, and measured in months at time of testing.

No.	Age	Aetiology of brain damage	Time since onset	Side	Site of lesion	YOE	NART FSIQ
P1	50	Meningioma, surgery	54 mths	L	Parietal convexity	10	110
P2	51	Low grade intrinsic tumour, surgery	66 mths	R	Temporal, extensive infiltrating	15	98
P3	46	Med. grade intrinsic tumour, surgery	20 mths	R	Parietal, extensive cortex & white matter	11	94
P4	58	Meningioma, excised	46 mths	R*	Upper occipital*	10	95
P5	41	Excision of epileptic focus	62 mths	R	Medial temporal	13	112
P6	44	Vascular (cerebral haemorrhage & surgery)	60 mths	R	Parietal convexity, large; minor FL extension	10	86
P7	37	Excision of epileptic focus	12 mths	L	Medial temporal	14	102
P8	55	Closed head injury	36 mths	R	Occipital	10	100
P9	38	High grade intrinsic tumour, surgery	6 mths	L	Temporoparietal	11	86
P10	49	High grade intrinsic tumour, surgery	2 mths	L	Occipitoparietal	17	101
P11	43	Low grade intrinsic tumour**??**, surgery	30	L	Temporal, mainly medial, extending to insula	14	102
P12	49	Med. grade intrinsic tumour, surgery	6 mths	R	Medial temporal, extending to external capsule, large	12	107
P13	58	Vascular (cerebral infarction)	5 mths	L	Occipital including primary visual area	11	103
P14	75	Vascular (cerebral haemorrhage)	9 mths	L	High occipitoparietal, convexity	14	112

Table 5.1b. Clinical details of posterior lesion group. See Table 6.1a (above) for abbreviations. *According to pre-op scan; but reading difficulties suggested L occipital involvement; treated as bilateral.

There were 14 patients in each group. The inclusion criterion for the frontal patient group was that at least three quarters of the lesion should be anterior to the central sulcus, as visible on a CT or MRI scan. For the posterior patient group, the converse criterion was applied, that at least three quarters of the lesion should be posterior to

the central sulcus. In each group, there were 7 patients with left sided lesions, 6 patients with right sided lesions, and one with a bilateral lesion. In the frontal lesion group, there were 6 women and 8 men, and in the posterior group, 7 women and 7 men. There were 3 left-handed frontal lesion patients, but all the posterior lesion patients were right-handed.

Healthy control group

14 healthy controls were also included in the study. These were matched on age and years of education with the patient groups. 12 were recruited from the volunteer panel of the Department of Experimental Psychology. They were paid a standard rate for participation. A further 2 particpants were partners of patients in the study, and were not paid for participation. There were 9 women and 5 men, and their mean age was 56yrs. Their mean number of years of education was 13. 4 were left-handed.

Tasks and apparatus

Neuropsychological baseline and executive function testing

A short battery of standardised neuropsychological tests was given to participants. Some screened for specific cognitive deficits, and these were given only to patients. All participants of age 70 years or above were also given the Folstein Mini Mental State examination, to exclude significant concurrent dementing illness. All were also screened for depression and anxiety using the Hospital Anxiety and Depression Scale, with the aim of ensuring there were not major differences between groups. The other tests aimed to assess specific executive functions (for details of their purpose see Introduction, above). These were given to all participants, with the exception of the Wisconsin Card Sort Test, which was only given to patients. The reason for the exception was partly because perseveration was mainly of interest in the patients (see Introduction), but also because of time constraints a few of the older controls on the volunteer panel had previously been given a different version of the WCST. The procedure for each test is described briefly.

Screening and neuropsychological baseline tests

These were employed to assess patients' cognitive state in a number of respects considered essential for them to be able to take part in the study. Results are therefore

not reported in full.

1. **The Folstein Mini Mental State examination (MMS) (Folstein, Folstein et al. 1975).** This quick simple screening test was carried out in participants of 70 or over only, to screen for significant dementing illness.

2. **The Hospital Anxiety and Depression Scale (HADS) (Zigmond and Snaith 1983).** This is a brief questionnaire assessing state anxiety and depression, each on a scale of 0 to 20, without employing questions likely to be affected by concurrent physical illness. The cutoff for inclusion in the study was a score of >= 15 on either section, suggesting severe anxiety or depression.

3. **The National Adult Reading Test (NART) (Nelson and O'Connell 1978).** This test estimated premorbid IQ from current reading vocabulary.

4. **The Mini Minnesota Aphasia Screening test (Powell, Bailey et al. 1980).** This brief test looked at comprehension, reading, writing/ spelling and picture naming.

5. **The Star Cancellation test (from the Behavioural Inattention Test, Wilson, Cockburn et al. 1987).** This quick test screened for hemispatial neglect. Patients had to cross out all the small stars on a page of stars and other symbols.

6. **Forward and backward digit span.** This was taken from the Wechsler Adult Intelligence Scale III-R (Wechsler 1981). Patients received one point for each complete string of digits correct at spans between 3 and 8 (forward), and 2 and 7 (backward). For both backward and forward span, testing was discontinued if there was an error on both trials at one span length. This test was included mainly to check that patients' working memory capacity was sufficient to remember and follow the task instructions.

Frontal lobe and executive function tests

1. **The Dysexecutive Behaviour Questionnaire (DEX) (Wilson, Alderman et al. 1996).** This consisted of two parts. One was given to the patient to complete, and they were asked to pass the second to a close relative, friend or carer. Both entailed 20 questions about the patient's behaviour patterns.

2. **The Wisconsin Card Sorting Test (Grant and Berg 1948).** The full 128 card test was administered. The procedure for this has been described in some detail in the Introduction to this study.

3. **The Controlled Oral Word Association 'FAS' (Verbal Fluency) Test (Benton 1968).** Participants were asked to generate as many different words from each of the three letters F, A and S, with a minute for each. Repetitions, proper names and variations of the same word with different endings were not allowed. The number of correct words produced in the total 3 minutes, and the number of perseverative errors, was recorded.

4. **The Trail Making Test (Armitage 1946).** After brief practice to ensure instructions were

understood, participants were presented first with a sheet of paper with circled numbers to be joined by drawing a continuous line, and then with a sheet with circled numbers and letters. They had to follow a 'trail' of numbers (part A) and then of alternating numbers and letters (part B) by connecting each item in the trail in turn, as quickly and accurately as possible. The test was scored in terms of number of perseverative errors on part B (i.e. two numbers or two letters joined consecutively), and the time taken for each part. The score of interest here was the time taken on part B, including time for correcting any errors (Reitan's scoring method, see Lezak 1995).

5. **The Stroop Neuropsychological Screening Test (Trenerry, Crosson et al. 1989).** In this test, all stimuli were incongruent, and consisted of the colours and words blue, red, tan and green. Participants first read colour words aloud for 2 minutes, then named ink colour from a similar stimulus sheet for a further 2 minutes. The 'colour-word' score given was the number of correctly named ink colours minus the number of errors. Word reading was not scored.

Experiment 6a: Speeded response switching test

Participants had to switch unpredictably between the same two subtasks, picture classification and word classification, that were used in Experiments 4, 5a and 5b (for details, see Method for Experiment 4, Chapter 3).

Experiment 6b: Perseveration test

This was based on Sandson and Albert's (1984) stuck-in-set test, as described in the Introduction. Stimuli consisted of a single page of equal numbers of squares and circles arranged in rows. There were boxes beneath the stimuli for responses. There were three parts:

1. **Establishing task set.** Particpants wrote the word 'square' or 'circle' below the relevant stimuli. The number and type of errors were measured.

2. **Copying.** Participants copied the shapes into the boxes. Time for completion of one sheet of stimuli, and number and types of errors were measured.

3. **Reversal.** Circles were drawn under a square, and vice versa. Time for completion of one sheet of stimuli, and number and types of errors were measured.

Design

The overall design of this study (including baseline testing and both experiments) was between-groups. This meant that the only effects directly of interest for analysis were differences between patient groups and between patients and controls.

Experiment 6a: Speeded response test

The experiment manipulated 4 main independent variables in a 3 x 5 x 2 x 3 mixed design. These were patient group (frontal lesion/ posterior lesion/ control) task switching/ repetition (switch/ 1^{st} / 2^{nd} / 3^{rd} – 4^{th}/ 5^{th} – 8^{th} repetitions, as before), cue-target interval (short/ long) and stimulus valence (univalent/ bivalent-congruent/ bivalent-incongruent, as before). Apart from patient group, which was a between subjects factor, the rest were repeated measures factors varied within experimental blocks, as before. Otherwise, the design and counterbalancing were as in Experiment 4.

Experiment 6b: Perseveration test

There was one between-subjects variable, patient group (frontal/ posterior/ control), and two within-subjects variables, condition (establishing set/ copying/ reversal), and response sequence (repetition/ alternation). The latter indicated whether the required response was the same as, or different from, the one for the previous stimulus. In each part of the test, errors were therefore scored either as repetition errors, in which the response to the previous stimulus was inappropriately repeated, or as alternation errors, in which the response to the previous stimulus was inappropriately changed. The dependent measures were each participant's completion time for each condition (not analysed for establishing set), and their number of errors per page. The order of circle and square stimuli on the sheet was randomly assigned for each subtest separately, and for half of the participants, this order was reversed.

Procedure

Testing for patients was carried out in two or three sessions, depending on their ability and preferences. Its total duration was generally about 3 to 4 hours, including breaks. Testing took place either in the patient's home, in a clinic room at the hospital, or in the Department of Experimental Psychology, as the patient wished. Before formal testing commenced, patients were asked to sign a consent form, and a brief medical history was taken to confirm their suitability for the study. For healthy controls, testing took about 2 hours, and was carried out in the Department. In all cases, the task switching computer test was administered in the last (second or third) session, along with Experiment 6b. The test battery procedure is summarised in **Appendix C**.

Standardised tests

The order of administration of standardised tests was that screening tests were presented first, to ensure suitability for the study, then executive function tests. Within this, the order was usually that given in **Appendix C**, but varied to some extent depending on patients' general state of health and time constraints.

Experiment 6a: Speeded response switching test

The procedure for this study was based on that of Experiment 4, with the following differences. The response-cue interval was not manipulated, so was determined by the CTI (150ms or 1500ms), as the inter-trial interval was kept constant at 3000ms.

The experiment consisted of 10 blocks of 49 trials each, of which the first was not recorded, and on which CTI was always long. Participants were encouraged to take short breaks between blocks, and always after every second block. The task on each trial was determined randomly, such that there was a 0.25 probability of a task switch and a 0.75 probability of a task repeat. Other trial variables were then randomly selected with equal probability.

The practice procedure was of variable length, as it was considered important to ensure that patients of varying age and ability had time to learn the task. All participants were given a chance to practice the word and the picture subtasks separately, and then practice switching between the two. This was done in blocks of 12 trials, until the examiner was satisfied that the individual had learnt the test and making only occasional errors. Cues were presented during practice, although the task was constant, and CTI was varied as in the experiment.

Experiment 6b: Perseveration test

The three subtests for this were administered always in the same order, as given under 'Tasks and Apparatus' above. Patients were asked to complete each part as quickly and accurately as possible, and each was timed separately with a stopwatch (although times for the first part were not analysed).

Results of standardised tests

General screening

All patients included in the study performed within normal limits on the MMS (where appropriate) and the star cancellation tests. In the frontal lobe group there were three patients with some signs of language processing difficulties, as assessed by the Mini Minnesota screening test. Two posterior patients also showed significant and two showed minor impairments of language function. The degree of minor language impairment thus seemed to be roughly similar in the two patient groups, if anything slightly more marked in the posteriors.

Matching of groups

The two patient groups were compared on age, years of education and NART estimated premorbid full-scale IQ to assess the adequacy of matching. The data for individual patients are given in **Table 5.1a and b** (in Methods section). The average ages of the frontal and posterior lesion groups were both 54 years. The mean number of years of education was 13 for the frontal and 12 for the posterior lesion groups. T-tests showed that the two patient groups did not differ reliably on either of these variables ($t(26) = 1.00$, n.s.; $t(26) = 0.26$, n.s., respectively). The frontal group, however, scored significantly better on the NART ($t(26) = 2.79$, $p < 0.05$). Mean estimated IQ was 112 in the frontal versus 101 in the posterior group. This could to some extent have resulted from the significant acquired language difficulties of three posterior patients (P9, P10 and P4) versus one frontal patient (F14). However, despite this it was likely that the frontal lesion group were generally more able than the posterior lesion group. This was considered acceptable given that the study predicted deficits in the former group, and the main aim of matching was to avoid falsely attributing differences in general ability to specific impairments. It is noted at this point that this would not apply to any finding of selective deficits in the *posterior* group (see General Discussion).

Tests of executive function

The results of the standardised tests of frontal lobe and executive function are given in **Table 5.2a to c**, below. These were included in the study to see whether there was evidence of executive dysfunction in the patient groups, as well as to test predictions

about correlations of function. The performance of the different participant groups overall was compared with each other and controls, and group means and statistical tests for differences are shown in **Table 5.3**. Performance was also assessed in individual patients and controls using published test norms, to determine the presence of executive dysfunction. Unless otherwise stated, a score below the 5th centile for the normal population is taken as a sign of impairment. These are highlighted in **Table 5.2**. In this context it is noted that the use of age-controlled norms, particularly for the WCST, might mean that age-associated executive deficits are counted as normal performance. The main aim here with individual scoring, however, was not falsely to attribute an impairment to an individual.

	Side of lesion	FAS total words	WCST no. of cats.	WCST psv. resp.	WCST trials to 1st cat.	Stroop colour-word score	Trails 'A' time	Trails 'B' time	DEX self-rating	DEX ind.-rating
					Frontal lesion group:					
F1	Bil	25	**	**	**	65	104	**240**	6	21
F2	R	76	**3**	**25**	12	98	45	91	17	12
F3	L	30	6	16	**29**	119	27	62	36	7
F4	L	25	3	17	17	76	43	114	12	4
F5	L	36	**3**	**43**	11	58	53	90	26	57
F6	L	33	1	27	13	61	82	178	40	49
F7	R	70	**2**	30	12	121	29	48	10	9
F8	L	41	6	11	11	131	39	82	1	15
F9	R	39	**2**	33	11	100	42	80	21	15
F10	R	50	6	6	13	145	40	49	20	9
F11	L	33	6	13	12	118	49	73	7	5
F12	R	47	6	6	11	102	32	66	22	20
F13	R	45	6	8	11	119	24	40	15	21
F14	L	**10**	**3**	26	21	95	47	**157**	22	29

Table 5.2a. Executive function test results for the frontal lesion group. 'Bil' = bilateral lesion. For the WCST, the number of categories achieved, and the total number of perseverative responses are given. For the Trails test, only the score for part B (alternation) is given. Scores highlighted in bold represent impaired individual performance compared with published norms, usually indicated by a score below the 5th centile (see text for details). '**' indicates missing data (Patient F1 refused the WCST).

	Side of lesion	FAS total words	WCST no. of cats.	WCST psv. resp.	WCST trials to 1st cat.	Stroop colour-word score	Trails 'A' time	Trails 'B' time	DEX self-rating	DEX ind.-rating
Posterior lesion group:										
P1	L	39	3	29	12	123	46	98	20	17
P2	R	45	4	26	12	104	33	77	15	13
P3	R	41	1	28	14	87	31	59	18	6
P4	R/bil	17	3	**56**	12	**54**	**63**	99	24	44
P5	R	41	6	6	11	117	44	66	40	24
P6	R	36	4	23	12	97	31	78	15	0
P7	L	43	4	**39**	10	136	25	54	23	**
P8	R	**15**	3	60	10	75	37	140	15	15
P9	L	20	6	20	19	**66**	48	96	16	14
P10	L	34	6	13	10	81	39	107	12	10
P11	L	**	6	13	12	84	23	62	17	13
P12	R	29	4	35	12	111	**92**	**183**	19	7
P13	L	44	6	15	12	81	40	102	7	24
P14	L	24	0	**93**	n/a	**42**	107	138	26	33

Table 5.2b. Executive function and frontal lobe test results for the posterior lesion group. See above for details and abbreviations, apart from 'n/a': 'not applicable'.

Wisconsin card sorting test

This test was only administered to the two groups of patients. Looking at average performance (see **Table 5.3**) overall, there was no difference in the mean number of categories achieved, and the trend towards a difference in the number of perseverative responses was due to poorer performance in the *posterior* lesion group. This conclusion was not altered if percent perseverative responses was examined instead. Examination of individual patients' scores (see **Table 5.2**) showed that some did show deficits on the test, according to standard criteria. There were more of these in the frontal lesion group, including patient F2. Although not below the 5th centile, his performance was impaired compared to expectations, given his NART score and years of education (category score was at below the 10th centile, and perseveration at below the 20th). However, there was no difference between the two groups in apparent type (as well as degree) of impairment. No patient failed to maintain the current task set on this test.

	Side of lesion	FAS total words	WCST no. of cats.	WCST psv. resp.	WCST trials to 1st cat.	Stroop colour-word score	Trails 'A' time	Trails 'B' time	DEX self-rating	DEX ind.-rating
					Healthy controls:					
C1	n/a	40	**	**	**	88	33	94	8	8
C2	n/a	60	**	**	**	69	41	98	13	12
C3	n/a	39	**	**	**	**56**	47	90	9	6
C4	n/a	55	**	**	**	81	35	72	22	24
C5	n/a	57	**	**	**	108	23	76	16	8
C6	n/a	63	**	**	**	121	38	84	22	35
C7	n/a	54	**	**	**	106	29	77	16	13
C8	n/a	51	**	**	**	145	14	41	13	**
C9	n/a	**	**	**	**	161	19	53	14	10
C10	n/a	65	**	**	**	117	21	48	26	**
C11	n/a	32	**	**	**	104	41	74	27	26
C12	n/a	60	**	**	**	112	20	48	10	5
C13	n/a	57	**	**	**	**	24	46	9	**
C14	n/a	45	**	**	**	132	**	**	1	0

Table 6.2. Executive function and frontal lobe test results for the healthy control group. These are given in the same format as for the patients, for ease of comparison, although, as stated earlier, the WCST was not administered to the healthy controls.

Verbal fluency

There was no overall impairment of verbal fluency in the frontal lesion group, since the mean number of words produced was actually greater in the posterior group (**Table 5.3).** Using norms for verbal fluency with adjustments for age and years of education, only one patient in each group showed significant impairments[8] (**Table 5.2**). However, as performance on the FAS verbal fluency test is known to be strongly influenced by verbal intelligence (Lezak 1995), it was possible that the lack of an overall group difference was due to the fact that the frontal lesion group were more able than the posteriors in this respect. To avoid this confound, a theoretically motivated comparison can also be made of verbal fluency performance *within* the frontal group, since there is evidence of a specifically left frontal association with

[8] The posterior patient with an impairment had a right occipital lesion, so the cause for this was not immediately apparent, though the deficit could have related to more diffuse brain damage from his closed head injury that was not visible on the CT scan.

deficits on this test (Lezak 1995). A t-test was therefore carried out comparing the FAS performance of those frontal patients with clearly lateralised lesions on the left and on the right. Patient AP was excluded from the analysis to ensure that a positive group result was not just due to her dysphasia. Despite the small numbers (12), results showed a significant difference between the two groups, with the left frontal lesion patients performing worse than the right frontal patients (mean FAS scores were 33 and 55 correct words per minute; $t(10) = 3.32$, $p < 0.005$). The two groups did not differ significantly in their NART IQ scores ($t(10) = 1.02$, n.s.).

	FAS total words	WCST no. of cats.	WCST no. of psv. resp.	WCST trials to 1st cat.	Stroop colour-word score	Trails 'A' time (secs)	Trails 'B' time (secs)	DEX self-rating	DEX ind.-rating	DEX diff. score
Frontal	40	4	18	14	101	47	98	18.2	19.5	1.2
Posterior	33	4	33	12	90	47	97	19.1	16.9	-1.8
Control	52	**	**	**	108	30	69	13.7	13.4	-2.5
$F(2, *) =$	6.94	< 1	3.87	< 1	1.42	3.32	2.10	< 1	< 1	< 1
p <	0.005	n.s.	0.1	n.s.	n.s.	0.05	n.s.	n.s.	n.s.	n.s.

Table 5.3. Comparison of performance of patient and control groups on standardised tests of exective function including the Dysexecutive Behaviour Questionnaire. Mean scores on the various tests are shown. The statistics given are for an overall 1-way ANOVA comparing performance in all three participant groups. Follow-up tests are given in the text, below. (*Note that error df differ because of missing data in scores for some tests).

Stroop and trail making tests

On this test, all of the frontal lesion group performed within normal limits. Three posterior patients showed deficits compared to age-adjusted norms (Trenerry, Crosson et al. 1989), and a fourth showed borderline performance. One elderly healthy control also performed just below the cutoff given for over age 50yrs. There was clearly no evidence of a frontal lobe deficit, and no sign of a specific left frontal deficit (see e.g. Perret 1974), since although left frontal patients performed worse than right frontals (mean scores were 95 and 113, respectively), it can be seen that the latter performed better than the control group.

In the Trail Making Test, there was no difference between frontal and posterior lesion patients overall, both groups taking almost exactly the same time on average for both parts of the test (see **Table 5.3**). The reliable group difference on part A resulted from faster performance by the controls than by the frontal group (for Games-Howell test, p

< 0.05 for this comparison; comparing posteriors and controls, 0.05 < p < 0.1; for frontals and posteriors, n.s.). However there was evidence of individual impairment in two frontal patients. Both showed a selective slowing on part B, and one (patient F1) also made a perseverative error on Trails B (for norms see Lezak 1983).

Assessment of dysexecutive behaviour

One aim of the present study was to explore the links in this study between test performance, particularly task switching, and behaviour. It can be seen from **Table 5.3,** though, that the three experimental groups did not differ reliably on scores on either the self- or the independent- rater parts of the DEX questionnaire. The two patient groups scored slightly higher on both ratings, but this difference was not significant. This could have been due to the presence of dysexecutive behaviour in some individuals in both patient groups, since although dysexecutive behaviour is often reported in patients with focal damage to the frontal lobes (Rylander 1939; Stuss and Benson 1986), it does not occur in all such individuals, and nor is it confined to this group[9]. A closer look at the data, however, does not reveal any support for this, no individual patients scoring in the 'impaired' range (Wilson, Alderman et al. 1996).

In the original study of the DEX questionnaire, the measure of behavioural impairment that best predicted impairment on tests of executive function was the *difference* between the independent rating of the patient's performance and their self-rating (Wilson, Alderman et al. 1996; Burgess, Alderman et al. 1998). In the present study, however, this measure also did not reveal group differences. Neither of the patient groups showed significant dysexecutive behaviour as operationalised in this way. Pearson product moment correations were calculated between the difference score and four standardised scores of interest, to see whether the pattern of individual differences would replicate Wilson et al's findings that executive dysfunction (regardless of lesion site) predicted behaviour. They found reliable correlations with perseveration on a modified version of the WCST, with FAS score, and with Trails B (but not A) (the Stroop test was not included in that study). Results from the present study showed a significant correlation of the DEX difference score with Stroop

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[9] Anecdotal evidence suggests that it is commoner in patients with closed head injury, who may have diffuse damage to white matter tracts connecting the frontal lobes to other brain regions (see e.g. McKinlay and Gray 1992).

performance ($r = -0.427$, $p < 0.01$; $N = 39$), and with *both* parts of the Trails test (for A, $r = 0.378$, for B, $r = 0.358$, $p < 0.05$ for both; $N = 39$). There was some trend towards a correlation of the DEX difference score with WCST perseverative responding ($r = 0.335$, $0.05 < p < 0.1$; $N = 26$), but no significant relationship with verbal fluency scores ($r = -0.160$, n.s.; $N = 38$). The only correspondence between findings of the present study and those of Wilson et al (1996) was therefore in the marginal relationship between WCST performance and behaviour. The lack of specificity in the correlations with part B of the Trail Making Test here suggests a different processing basis to the correlation found in the original study.

Summary

There was therefore some evidence for deficits of executive function in the patients in this study, but in group comparisons this did not localise to the frontal lobes. An exception to this was evidence of a reduction in verbal fluency in those with left compared to right frontal lesions. More individuals with anterior than posterior brain lesions showed impairments on one or more of the tests. Discounting patient P9's Stroop performance as likely to be secondary to her language deficits, there were seven such in the former group and three in the latter. Looking at the WCST in particular, as a putative test of task switching, abnormally high rates of perseverative responding were just as common in the non-frontal group. It was therefore decided not to attempt to correlate individual differences in task switching performance with executive function tests, since it would not be clear to what extent variance in the non-executive processes involved would account for any positive findings. The task switching data, however, would be inspected to see whether any particular pattern pertained in those patients with deficits in card sorting (see below). The same problem applied to the prediction of behaviour on the DEX from switching performance, and this was also not examined further. Broader implications of these findings for the study of executive function will be considered in the General Discussion.

Results of experiment 6a

The speeded-response switching task tested specific predictions concerning differences between experimental groups. Overall performance will not be described in detail, except where data run counter to the patterns established in Experiments 4, 5a and 5b. Three separate analyses were carried out. The first concerned task switching and repetition performance with and without preparation and interference between tasks, the second examined the effects of interference on the previous trial, and the third looked at response repetition effects.

Switching to a new task, and the use of cues

An analysis was first carried out of task switching and repetition performance. As in Experiments 4 and 5, the data were separated into bins according to successive task repetitions, and excluding task repetitions after the 8th. Data from all participant groups were analysed together. A mixed model ANOVA with the following factors was carried out on median RT and error proportions: Patient group (frontal/ posterior/ control), task switching/ repetition (switch trials, then 1st/ 2nd/ 3rd- 4th/ 5th- 8th task repetitions), cue-target interval (CTI) (short/ long), and stimulus valence (univalent/ bivalent-congruent/ bivalent-incongruent). In addition, to test certain specific predictions, planned comparisons were carried out on data corrected for overall speed and accuracy. Detailed analysis will only be presented of effects involving patient group.

The data are illustrated in **Figure 5.1** and **5.2**. **Figure 5.1** shows the effects of preparation on task switching and repetition in the three groups of participants, giving long and short CTI data separately. **Figure 5.2** shows the effects of task repetition and stimulus valence in a similar fashion. The pattern of findings appears to be similar in all three experimental groups, despite slower overall RT for the posterior lesion group than for the other two groups. Because of this, the overall analysis will first be presented briefly, then specific predictions and group differences examined.

It is noted that one elderly posterior lesion patient (no. 42) found the task particularly difficult and made about 12% errors overall, as well as being slow. The data were explored with him excluded from analysis, but the pattern of findings with respect to groups was unaffected, except that the general performance deficit in the posterior

group was reduced. Data analysis has therefore been presented for all participants.

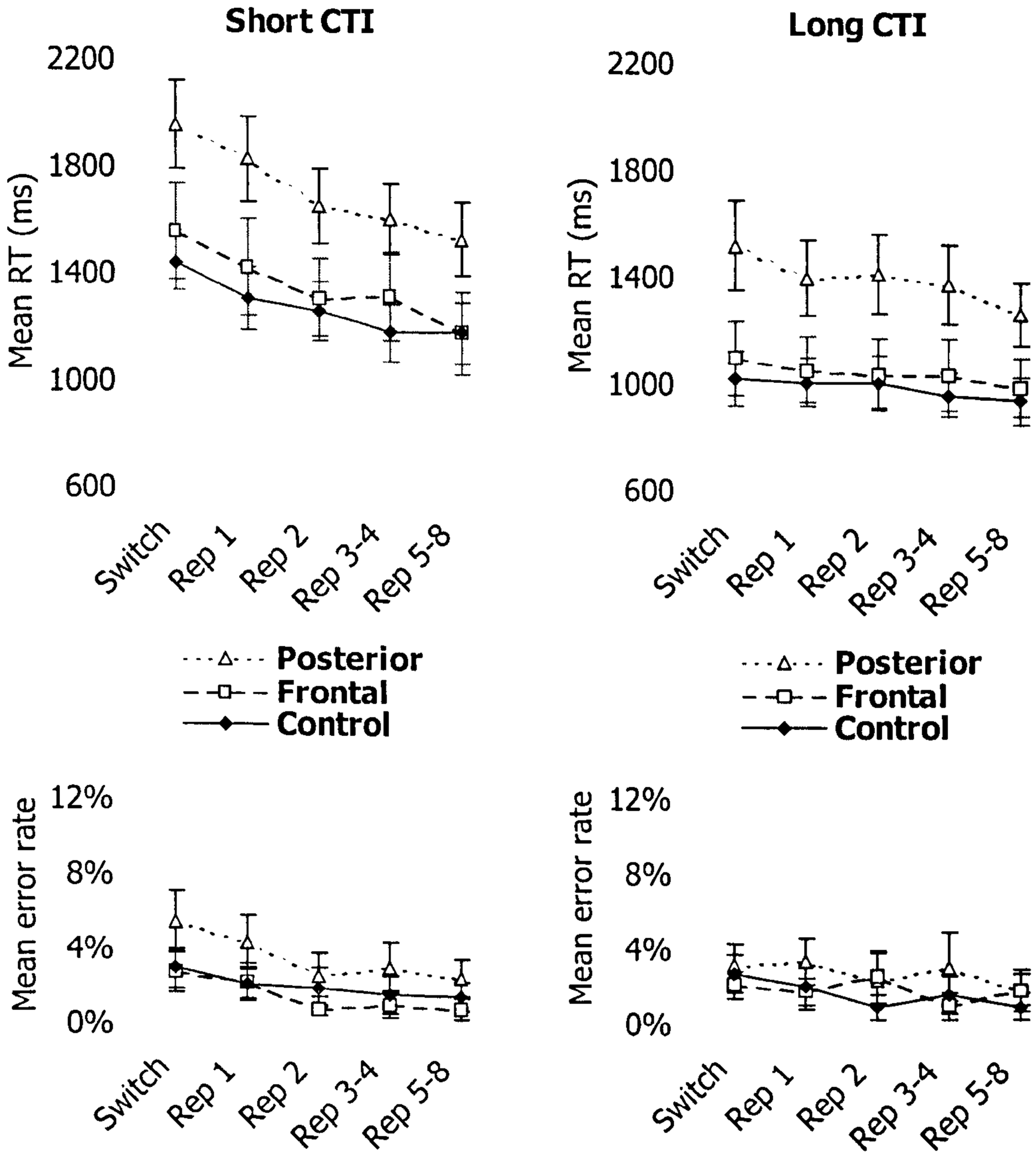

Figure 5.1. Task switching and repetition performance showing the effects of preparation, in Experiment 6a. The data for the three participant groups are illustrated separately. Error bars represent the standard error of the mean.

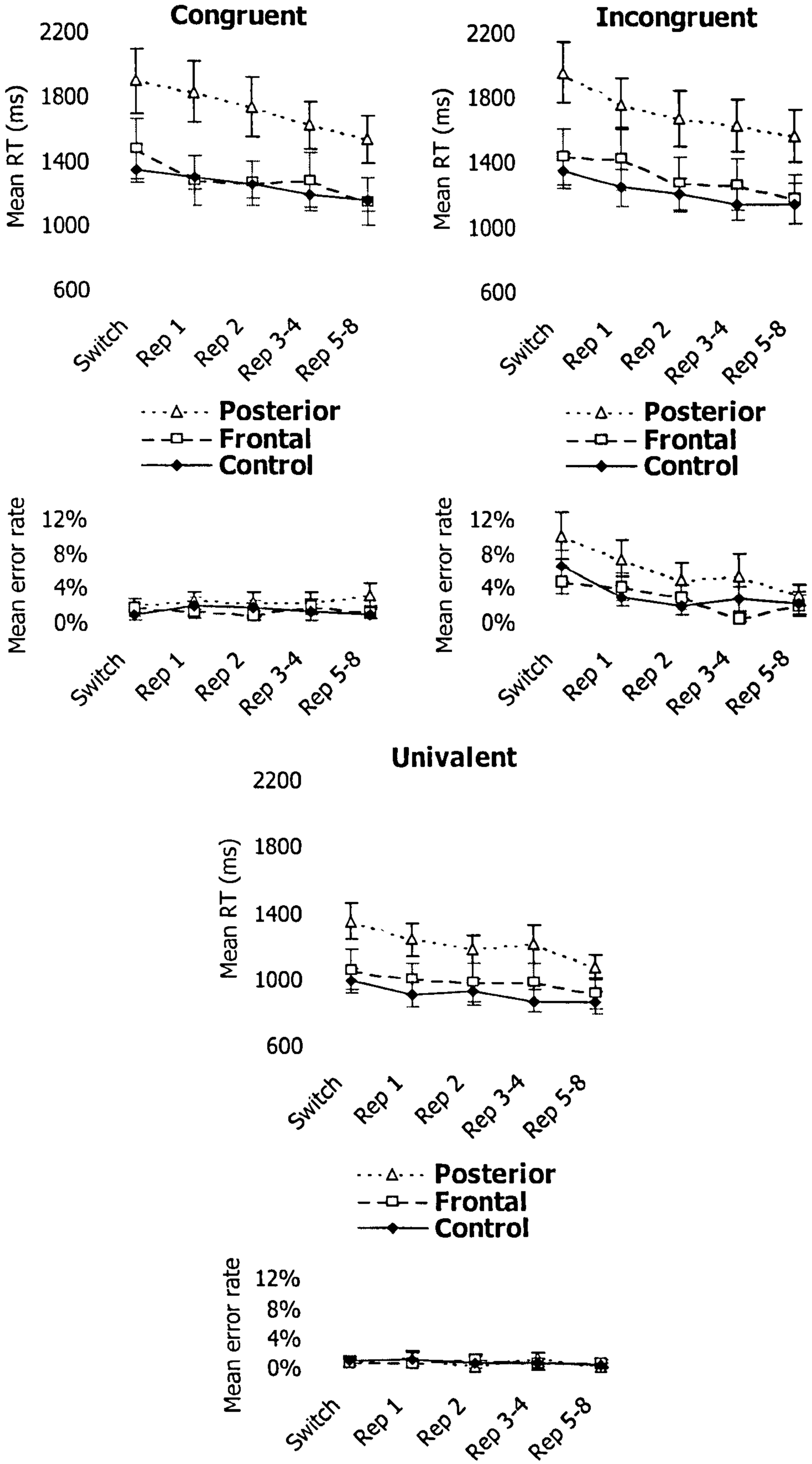

Figure 5.2. Task switching and repetition performance showing the effects of interference between tasks associated with stimulus valence, in Experiment 6a. The data for the three participant groups are illustrated separately. Error bars represent the standard error of the mean.

Overall analysis

As in the earlier studies of task switching and repetition in this thesis, a progressive RT speedup can be seen in **Figure 5.1** that appears to have been more marked at short than at long CTI. **Figure 5.2** shows that this effect was also more pronounced for bivalent than for univalent trials.

Analysis of RT revealed reliable main effects of task switching/ repetition ($F(4,156) = 44.09$, $p < 0.001$), CTI ($F(1,39) = 156.37$, $p < 0.001$) and stimulus valence ($F(2,78) = 91.73$, $p < 0.001$). There were reliable overall interactions of task switching/ repetition with CTI ($F(4,156) = 12.74$, $p < 0.001$) and with valence ($F(8,312) = 2.58$, $p < 0.01$), and of CTI with valence ($F(2,78) = 16.38$, $p < 0.001$). The 3-way interaction of switching/ repetition with CTI and valence was not significant ($F < 1$, n.s.). For errors, there were significant main effects of task switching/ repetition ($F(4,156) = 6.53$, $p < 0.001$) and of valence ($F(2,78) = 22.27$, $p < 0.001$). Task switching/ repetition interacted significantly with stimulus valence ($F(8,312) = 6.62$, $p < 0.001$), but CTI did not ($F(4,156) = 1.69$, n.s.). There were no other reliable effects on errors.

For all groups considered together, progressive improvements in RT with successive repetitions of the same task were found. This improvement was greater at short than at long CTI (for interaction between linear decreasing trends in RT, $F(1,39) = 20.43$, $p < 0.001$). It was also greater in the presence of task-irrelevant interference (comparing univalent and congruent trials $F(1,39) = 11.24$, $p < 0.005$; for comparison with incongruent trials $F(1,39) = 31.47$, $p < 0.001$). These findings replicated the results of Experiments 4, 5a and 5b. The error data generally supported the RT findings in both cases, again as in the previous studies.

Residual switch costs

For all groups together, there was a trend towards a reliable effect of task switching and repetition at long CTI ($F(1,39) = 4.30$, $0.025 < p < 0.05$, adjusted $\alpha = 0.025$), but not when only univalent trials were analysed ($F(1,39) = 3.24$, n.s.). Although the simple interaction effect of task repetition and group at long CTI was not significant ($F(2,39) = 1.12$, n.s.), post hoc tests were carried out to see whether there were interesting differences at long CTI, because of the importance of the residual switch cost. These showed some trend towards a simple main effect of task repetition at long CTI for posteriors for bivalent and univalent trials together ($F(1,39) = 5.91$, $0.008 < p$

< 0.02), but not for univalent trials alone ($F(1,39) = 3.99$, n.s.). There was no hint of a residual cost by either criterion for the frontal group ($F < 1$, n.s. for both effects), or for the controls ($F < 1$, n.s. for both). There was no evidence of reliable error residual costs in any participant group.

Effects of lesion location on performance

It can be seen from **Figures 5.1** and **5.2** that the posterior lesion group generally responded more slowly and less accurately than the other two groups. In the overall ANOVA, there was a reliable main effect of patient group on RT ($F(2,39) = 3.39$, $p < 0.05$), a significant interaction of group with stimulus valence ($F(4,78) = 2.65$, $p < 0.05$), and a trend towards an interaction of group with task switching/ repetition ($F(8,156) = 1.88$, $0.05 < p < 0.1$). Patient group did not interact reliably with CTI ($F < 1$, n.s.), and there were no significant higher order interactions involving this factor (for 3-way interactions of group with switching/ repetition and CTI, and with CTI and valence, $F < 1$, n.s.; for 3-way interaction of group with switching/ repetition and valence, and for 4-way interaction, $F(16,312) = 1.08$, n.s. and $F(16,312) = 1.20$, n.s., respectively). For errors, there was some trend towards a main effect of patient group ($F(2,39) = 2.36$, $0.1 < p < 0.2$), a reliable interaction of patient group with CTI ($F(2,39) = 3.25$, $p < 0.05$), and a trend towards an interaction with valence ($F(4,78) = 2.21$, $0.05 < p < 0.1$).

Task-irrelevant interference and lesion site

Again looking at **Figure 5.2**, it appears that irrelevant stimulus attributes caused more RT slowing in this group, as reflected in the reliable interaction between these factors. Mean RT for the posterior lesion group was 1714ms on incongruent trials and 1206ms on univalent trials; for the control group, mean RTs were 1216ms and 908ms, respectively. Mean RT for the frontal lesion patients was 1312ms for incongruent trials, and 982ms for univalent trials.

Because of the slower performance in the posterior group overall, it was suspected that the apparent interference effect was secondary to this, reflecting an interference effect in proportion to the baseline response speed. For this reason, the RT data were 'scaled' by dividing each data point for each participant by that participant's 'baseline speed', or overall mean RT. An equivalent procedure was carried out for error data. The dependent variable was thus the mean RT or error rate as a *proportion* of the

overall average RT or error rate. As one participant made no errors, these data were counted as missing in the scaled error analysis (proportions being meaningless).

The interaction of patient group with stimulus valence on the scaled data (see above) was found to be no longer reliable ($F(4,78) = 1.33$, n.s.). The same was true of the main effect of patient group on RT ($F < 1$, n.s.).

Task switching/ repetition and lesion site

In a similar fashion, the trend towards an interaction of task switching and repetition with patient group, apparently due to a more rapid reduction in RT over successive task repetitions in the posterior lesion group than in the frontal patients or healthy controls, was not found when the scaled data were analysed ($F < 1$, n.s.).

Two planned comparisons were then carried out on the switching/ repetition data to test two specific hypothesis. To recap, it was predicted firstly that the frontal lesion group would show a more marked effect of task switching/ repetition at long CTI, because neglect of task-specific cues would lead to a failure of preparation. Analysis showed that after data scaling, the interaction of group with task switching/ repetition was not significant ($F < 1$, n.s.). This confirmed that patient groups did not differ reliably in terms of their use of cues for task- preparation during a long cue-target interval.

The second prediction was that patients with frontal damage would be 'stuck' in a switched-from task set, so would show more gradually reducing RT and error rates over the transitions from a task switch to successive task repetitions, in the *absence* of preparation, i.e. at short CTI. Inspection of **Figure 5.1** suggests that any trend towards a shallower decline in RT with task repetition in the frontal patient group was likely to be secondary to slower performance in the posterior lesion group. However, calculation of scaled effects was not necessary in this case, since the frontal patients' and healthy controls' performance did not differ, and the trend was in the wrong direction to be explained by an unexpected *posterior* deficit in switching.

Effect of CTI on errors by lesion site

The interaction of patient group with CTI for errors resulted from a difference between the frontal and the posterior patient groups. Mean error rates at short and long CTI were 1.4% and 1.8% for frontals. For posteriors, they were 3.5% and 2.7%, and for

controls 1.9% and 1.6%, respectively. Inspection of the data therefore suggested that preparation had a greater effect in terms of reducing errors in the posterior lesion group than in the others. However, analysis of scaled error data showed this to be due to a slightly higher overall error rate in the posteriors (for interaction of group and CTI, $F < 1$, n.s.).

Summary

The pattern of task switching and repetition performance overall in this study clearly resembled that found in Experiments 4, 5a and 5b, which used the same task and similar designs. However, surprisingly, there were no important differences in performance between the two lesion groups, or between patients as a whole and controls. There was no evidence that patients with frontal lobe damage failed to carry out task-specific preparation due to neglect of cues, and no evidence of their being stuck-in-set, as operationalised here. The patients with posterior lesions responded more slowly overall, and made slightly more errors than those with frontal lobe lesions, or healthy controls. An increase in the effect of task-irrelevant interference on RT in the posterior group was found to be secondary to slower overall performance. The same was found to be true of a greater reduction in error rates with preparation in the posterior group, when data were corrected for overall accuracy. However, a residual switching and repetition effect is noted in the posterior group only.

Inhibition of task set? Sequential stimulus valence analysis

The data were also analysed to explore the effects of previous trial stimulus valence on task switching and repetition performance in patients with frontal lobe and posterior brain damage. The raw data were reanalysed in the same way as reported for Experiment 4 (see Chapter 3). A mixed model ANOVA with the following factors was carried out on median RT and on error proportions: Patient group (frontal/ posterior/ control), Task switching (task switch, then 1^{st}/ 2^{nd}/ 3^{rd}– 4^{th}/ 5^{th}– 8^{th} task repeat), stimulus valence (univalent/ bivalent), and valence on previous trial (univalent/ bivalent). Only differences between participant groups concerning previous trial valence will be discussed in detail here. Error data are given, but the analysis not reported further as there were no reliable effects or important trends involving the variable previous trial valence.

The prediction for performance of the frontal lesion group concerned task switch trials.

If the findings of Rogers et al were replicated, RT in the frontal lesion group was expected to be slower if the previous stimulus had been bivalent, and the switch trial was also bivalent (Rogers, Sahakian et al. 1998). The data are shown in **Table 5.4**, and figures for task repetition trials are also given, for comparison. It is noted that the pattern of findings on the latter appeared broadly to be similar to the results of Experiments 4, 5a and 5b. The most important feature of this is that RT was generally faster on bivalent task repeat trials if the previous trial had also been bivalent.

Overall analysis of RT showed a reliable 2-way interaction of of previous stimulus valence with patient group ($F(1,39) = 11.44$, $p < 0.005$). The 3-way interaction of these two factors with task switching was not reliable ($F(2,39) = 2.25$, n.s.). The 3-way interaction of previous trial valence with patient group and current valence, and the 4-way interaction, were also not significant ($F < 1$, n.s. for both).

	TASK SWITCH				*TASK REPEAT*			
	Current univalent		**Current bivalent**		**Current univalent**		**Current bivalent**	
	RT (ms)	**% Errors**	**RT (ms)**	**% Errors**	**RT (ms)**	**% Errors**	**RT (ms)**	**% Errors**
				Frontal lesion group:				
Previous univalent	1009 (118)	0.6 (0.6)	1439 (199)	3.3 (0.9)	894 (80)	0.8 (0.4)	1311 (183)	1.9 (0.6)
Previous bivalent	1054 (121)	0.5 (0.4)	1457 (175)	3.1 (0.8)	978 (109)	0.7 (0.4)	1218 (151)	1.7 (0.4)
Previous valence effect	**45**	**-0.5**	**18**	**-0.2**	**84**	**-0.1**	**-93**	**-0.2**
				Posterior lesion group:				
Previous univalent	1301 (114)	0.0 (0.0)	1799 (187)	6.0 (1.5)	1074 (70)	0.9 (0.5)	1693 (163)	4.4 (1.1)
Previous bivalent	1401 (116)	1.2 (0.8)	1996 (201)	5.4 (1.7)	1187 (90)	1.0 (0.3)	1624 (148)	3.3 (1.1)
Previous valence effect	**100**	**-1.2**	**197**	**-1.6**	**113**	**0.1**	**-69**	**-1.1**
				Healthy controls:				
Previous univalent	924 (88)	1.4 (0.9)	1229 (114)	4.1 (1.0)	800 (56)	0.7 (0.3)	1166 (117)	2.0 (0.4)
Previous bivalent	920 (77)	0.7 (0.5)	1274 (127)	3.7 (0.7)	865 (71)	0.8 (0.3)	1113 (117)	1.8 (0.5)
Previous valence effect	**-4**	**-0.7**	**45**	**-0.4**	**65**	**0.1**	**-53**	**-0.2**

Table 5.4. Shows the effects of stimulus valence on the previous trial, on current trial performance for the three patient groups. The previous valence effect is shown separately for current univalent and bivalent trials, and is given as an *increase* in RT or in errors due to previous task-interference. Standard errors of the mean are shown in brackets.

Because the prediction concerned task switch trials, the trend towards a 3-way interaction of patient group with previous trial valence and task switching was followed up. This showed that there was a reliable simple interaction effect of patient group and previous trial valence on switch trials only ($F(2,39) = 4.19$, $p < 0.025$; for task repeat trials, $F < 1$, n.s.). Further analysis demonstrated that the posterior group differed significantly from both the frontal group (for simple interaction effect of patient group and previous valence, $F(1,26) = 4.93$, $p < 0.05$) and the controls ($F(1,39) = 5.31$, $p < 0.05$). Frontal lesion patients and controls did not differ ($F < 1$,

n.s.). The simple main effect of previous trial valence on RT was found to reliable for the posterior group only ($F(1,13) = 9.75$, $p < 0.01$; for frontals, $F(1,13) = 1.93$, n.s.; for controls, $F < 1$, n.s.).

Given this unexpected apparent performance deficit in the posterior lesion group, it was important next to determine whether this was attributable to their overall response slowing. An analysis was carried out using scaled RTs, as described in the previous section. The differences between groups were less clear cut, since the simple interaction effect of previous stimuls valence and group on task switch trials now just failed to reach significance ($F(2,39) = 2.61$, $0.05 < p < 0.1$). However, the simple main effect of previous trial valence on task switch trials remained significant for posteriors, but not for frontals or controls, despite a trend in the former group ($T(13) = -3.45$, $p < 0.005$; $T(13) = -1.86$, $0.05 < p < 0.1$; $T(13) < 1$, n.s., respectively). There was, therefore, still evidence of an effect of previous trial valence on switching for the posterior group. However, another possible reason for this could have been a difference in general ability, because this group were known to have lower estimated premorbid (NART) IQ than the frontal lesion group. The analysis was therefore repeated with NART IQ as a covariate. This showed that there was now no hint of a simple main effect of previous valence on switch trials in the posterior group taken alone ($F < 1$, n.s.).

Summary

This analysis showed a difference between the patient groups in the influence of previous trial stimulus valence on task switch trials, but not on task repeat trials. However, this was not the expected increased effect in patients with frontal lobe damage (Rogers, Sahakian et al. 1998). There appeared, instead, to be a deficit in the posterior lesion group similar to that predicted for the frontal lesion group. However, more detailed analysis revealed that, although evidence of this difference remained after correcting for overall speed, it was abolished once the patients' estimated premorbid ability was controlled for.

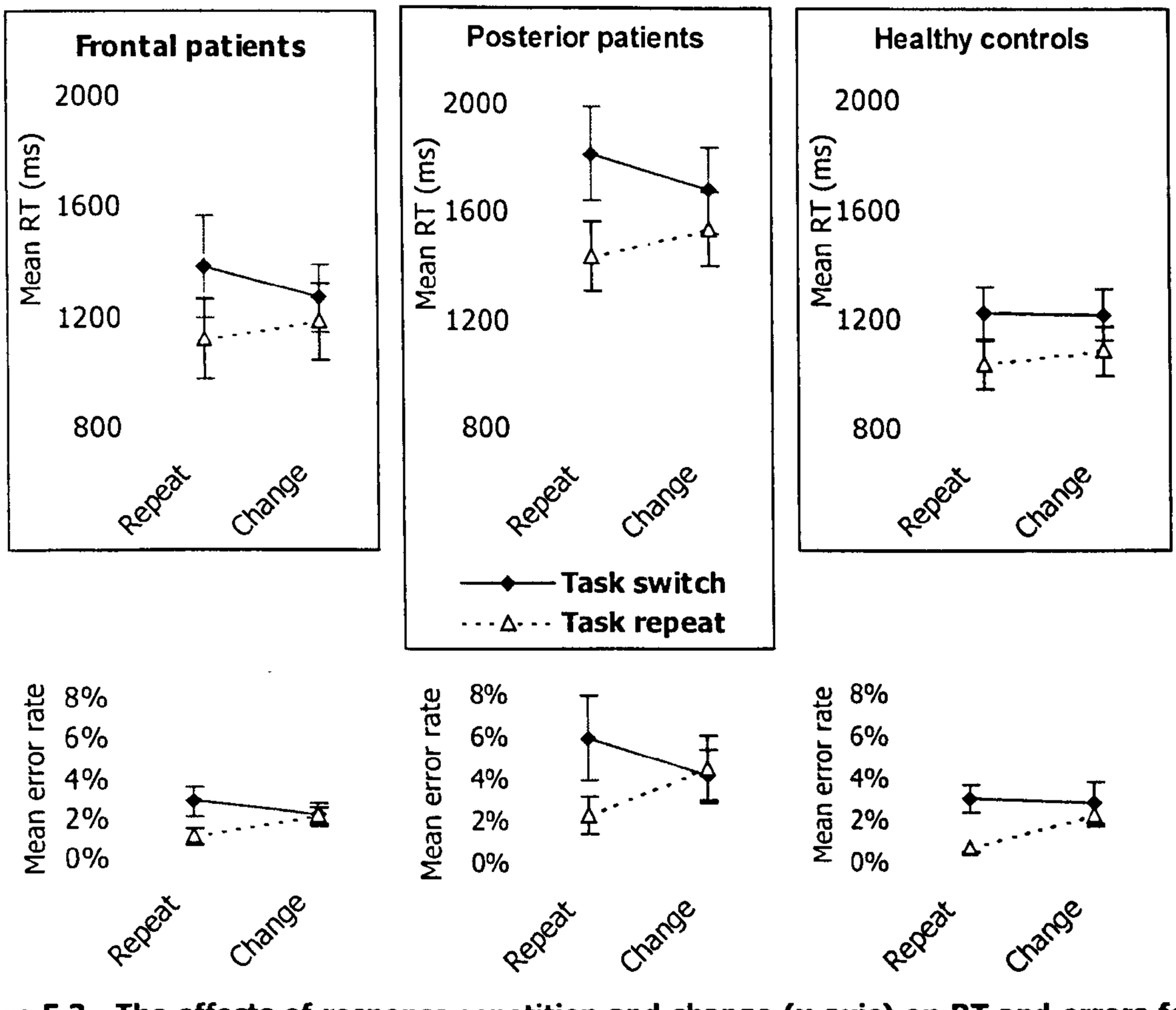

Figure 5.3. The effects of response repetition and change (x-axis) on RT and errors for task switch and task repeat trials in Experiment 6a. The data are shown separately for the three groups of participants. Error bars represent the standard error of the mean.

The relationship between the effects of task switching and repetition, and response repetition, were also examined in this experiment. The reason for this was to determine whether patients with frontal lobe damage would show impairments in control at the level of the individual response. To recap briefly, the following predictions were made for the frontal lesion group. On task switch trials, a tendency to repeat the previous response inappropriately would be reflected in more errors where the response should have changed, along with the absence (or reversal) of the usual paradoxical finding that response repetition leads to RT slowing when the task changes. On task repeat trials, an exaggeration of the usual effect in normals was expected, with more errors where the correct response changed from the previous trial, and more RT slowing.

The raw data were therefore reanalysed according to whether the response had been the same (response repeat), or different (response change), on the previous trial.

Since different successive task repetitions did not enter into the predictions, the task repeat trial data were analysed together. A mixed ANOVA with the following factors was carried out on median RT and error proportions: Patient group (frontal/ posterior/ control), task switching (task switch/ task repeat), and response repetition (response repeat/ response change). Only effects concerning differences between participant groups in response repetition will be described here.

The findings are illustrated in **Figure 5.3**, where data for the three experimental groups is shown separately. It can be seen that the pattern of their performance was broadly similar in terms of response times, allowing for slower overall RTs in the posterior patient group. For RT, there were no reliable differences between the patient groups (for the interaction between response repetition and patient group, $F < 1$, n.s.; for the 3-way interaction, $F(2,39) = 1.96$, n.s.). The same was true for error proportions (for interaction of response repetition and patient group, $F < 1$, n.s; for 3-way interaction, $F(2,39) = 1.28$, n.s.).

Summary of response repetition findings

In summary, this analysis did not reveal any deficits in frontal lobe, or posterior, lesion patients in terms of a tendency inappropriately to repeat the previous response. The pattern of pserformance of both patient groups was consistent with findings reported in the literature of better performance on response repetition trials when the task repeats, but paradoxically poorer performance when the task switches. Thus there was no support for the prediction that the patients with frontal damage would show a lesser decrement than posteriors and controls under these circumstances. There was also no sign of the exaggerated response repetition benefit, particularly in errors, predicted for the frontal group when the task repeated.

Comment on individual patient switching performance

The main method by which this study examined task switching performance was that of a between-groups comparison. This did not reveal any important overall differences between frontal and posterior lesion groups in terms of performance on the task switching test. There are a number of possible reasons for this, which will be considered in detail in the Discussion, below. However, one potential explanation was that only certain patients showed switching deficits, others being unimpaired in terms of executive function. This was plausible, given that a number of individuals,

particularly in the frontal group, did show evidence of executive impairment on standardised neuropsychological tests. As the Wisconsin Card Sort Test has been the most frequent basis for claims of switching deficits following frontal lobe damage, the data from individuals who failed this test were explored further. Their performance on the RT switching test was inspected and compared with that of the healthy control group. The aim was to see whether there were any important deviations in these individuals from the normal pattern of performance. This, on an individual level, was an extension of the approach described above (see Introduction), where associations between different tests purporting to have similar executive processing demands are of special interest. Performance on other tests of executive function was of lesser importance from this point of view.

The RT data for the six patients in the frontal group with deficits on the WCST are plotted in **Figure 6.4a**. Scaled data are given alongside those for the healthy control group, so that absolute position on the graphs can be compared to some extent. The most important observation is that there are no systematic deviations in the patients as a group from control performance. The only sign of a different pattern of performance in an individual patient is in patient F2, who shows, if anything, *steeper* RT speedup with task switching and repetition than controls. At long CTI, this might indicate some neglect of task cues, although there is only one outlying point (the switch trial). It would be rash to draw conclusions from one data point, particularly given the potential for strategic factors to influence individual performance. At short CTI, a deficit of the 'stuck-in-set' variety would be suggested by a *shallow* slope, the opposite of what the data show. There does not, therefore, seem to be any convincing evidence of switching deficits in the RT test specifically in those frontal lobe patients with impairments on the WCST. Patient F14 has two outlying points at short CTI, but these are on either side of the controls' data line.

Looking at the three posterior group patients who failed the WCST, in **Figure 6.4b**, the data are somewhat more difficult to interpret. P14 was the patient mentioned earlier who showed a particularly high rate of errors (12% overall; see above) suggesting that he was guessing the task rule on a high frequency of trials. His switching data are consistent with this in that little task-preparation appears to have taken place at long CTI, as reflected in similar RT slopes at long and short CTI (if anything *steeper* for long CTI). This pattern of performance could have been caused by a number of relatively general deficits, however, from a difficulty in controlling

interference to a failure to understand the instructions. Patient P4 appears to have performed better on task switch short CTI trials than controls, but was the second least accurate participant, with 7% errors overall. As for patient P14, most of these errors were on switch trials, so the RT pattern is probably secondary to inaccurate switching. The data are more consistent with a difficulty in switching in the posteriors than in the frontals as a cause of failures on the WCST, although they are a long way from confirming the former deficit.

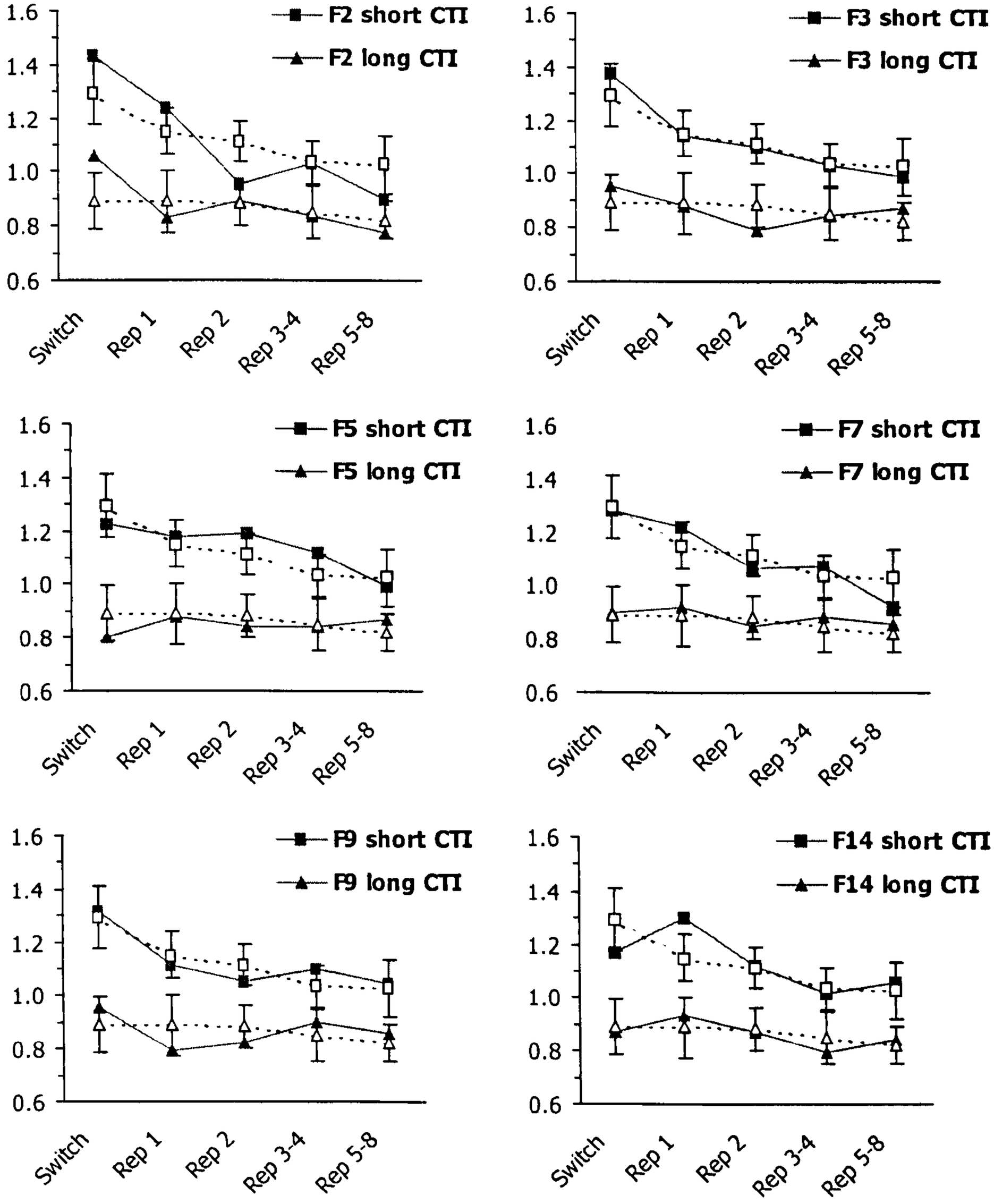

Figure 5.4a. Shows Experiment 6a data for the six patients in the frontal lobe group who failed the WCST by standard criteria. The open symbols and dotted lines show the control group data for comparison, with error bars representing the standard error of the mean. The open square sequence is short CTI for controls, and the open triangle sequence is long CTI for controls.

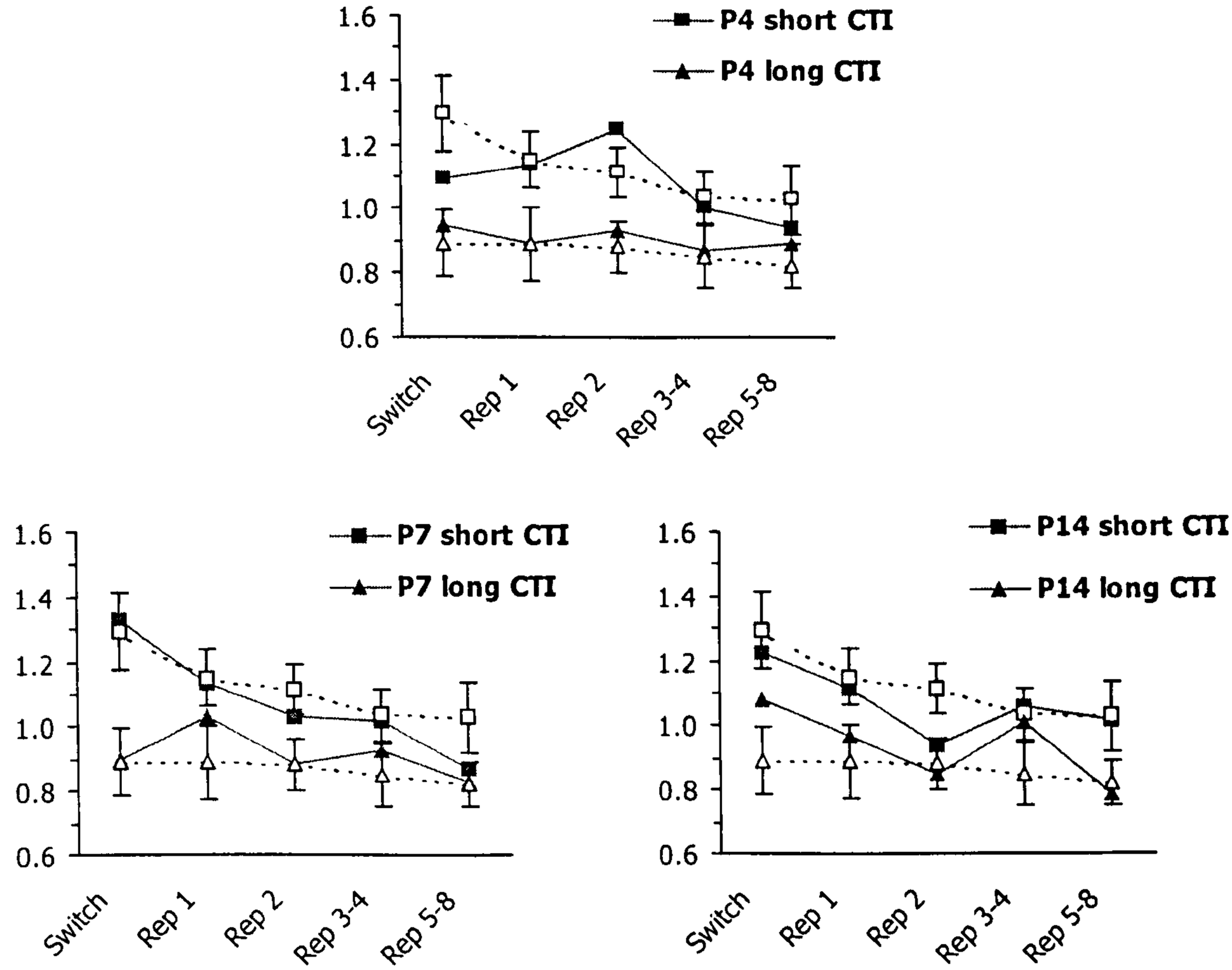

Figure 5.4b. Shows Experiment 6a data for the three patients in the posterior lobe group who failed the WCST by standard criteria. The open symbols and dotted lines show the control group data for comparison, with error bars representing the standard error of the mean. The open square sequence is short CTI for controls, and the open triangle sequence is long CTI for controls.

Expt. 6b: A pencil and paper test of perseveration

This brief experiment examined the performance of the patients and controls on the Sandson and Albert (1984) pencil-and-paper test of perseveration. The data of interest were the completion times for the copying and reversal conditions, and two types of errors. Alternation errors were those where the error response was different from the previous one, and repetition errors were those where it was the same. The data are given in **Table 5.5**. Individual scores are not shown, since no norms exist to interpret them. Errors were relatively few in number, and are represented as a the number of patients in the group who make one or more mistake, according to type,

rather than as rates per page, as originally intended. Completion times were not analysed for the 'establishing set' condition since these would largely reflect handwriting speed.

For this test, there were two predictions based on the presence of stuck-in-set perseveration or failures of response control, respectively. In the former case, slower performance and higher error rates were predicted in the reversal condition only (despite the fact that this was always performed last). In the latter case, repetition errors were predicted to be commoner than alternation errors in both copying and reversal conditions, regardless of any accompanying decrement in performance in the reversal condition.

	ESTABLISHING SET		SHAPE COPYING			SHAPE REVERSAL		
	Rep. errors	Alt. errors	Rep. errors	Alt. errors	Time to complete	Rep. errors	Alt. errors	Time to complete
Frontal	3	0	0	1	104	1	1	108
Posterior	8	7	3	0	91	6	2	103
Control	6	6	1	1	74	6	3	74

Table 5.5. Errors in Sandson and Albert (1984) stuck-in-set perseveration test. Table shows *frequency* of errors in each patient group. Note that data were missing for one frontal patient and one control. Times are in seconds. See text for details of error types.

To test the first prediction, the comparison between performance on copying and reversal, analysis of variance was carried out on the ratio of the completion time in the copying to the completion time in the reversal condition. This did not reveal any reliable differences between participant groups (mean ratios were 0.96 for frontals, 0.90 for posteriors, and 1.41 for controls; $F(2, 38) = 1.15$, n.s.). As can be seen, the control group's performance actually improved in the reversal condition, suggesting that any slowing due to reversal of task set was probably outweighed by a practice effect, since this condition was always administered last. With respect to the second prediction, inspection of the error data clearly indicates that while repetition errors were commoner than alternation errors in general, the frontal group performed *more* accurately than the other two, and with no predominance of either error type. There was also no evidence that posteriors were more prone to repetition errors than were the healthy controls.

General discussion

This study used a lesion method to explore the role of executive control in task switching. A group of patients with selective damage to the frontal lobes was compared with a similar group with selective brain damage posterior to the central sulcus, and their performance explored on the test of task switching and repetition developed in the previous two chapters. There were two broad predictions. The first was that frontal patients would have difficulty with the use of external cues for task-specific preparatory processing. The second was that they would have difficulty controlling interference from a competing task rule, this being distinguishable from impairments in inhibiting individual responses. In either case, the aim was to see whether such deficits were associated with broader impairments of executive function. The experiment also had a more clinical purpose, which was to determine whether such deficits in task or response control predicted dysexecutive behaviour. Overall, however, there was no evidence in the frontal group for impairments in the control of task switching. The implications will be considered for theories of task switching, and for appropriate methodology for the study of executive processing.

Characteristics of patient groups

The two groups of patients in this study were well matched on years of education, but those with posterior brain damage scored significantly worse than those with frontal lesions on the National Adult Reading Test. This probably indicated lower premorbid IQ, as well as slightly more marked acquired language deficits in the posterior group. In the RT switching test, the latter group also responded more slowly overall, compared with both the frontal and the healthy control groups. It is possible that this was the result of deficits in processes involved in sub-task performance, as opposed to switching or repetition, such as semantic processing. This seems unlikely, however, due to the varied lesion locations posterior to the central sulcus. It seems more plausible that the slower response times in the posterior patient group were related to the fact that the eight with tumours had higher grade lesions than the eight frontal lesion patients with tumours (see **Table 5.1**). These individuals were more likely, for example, to have significant brain swelling.

The standardised tests of executive function did not reveal any reliable differences between frontal and posterior lesion patients, despite the fact that a number of

individuals showed deficits on various tests. Of these measures, the most salient was the Wisconsin Card Sort Test, since this has been claimed to be a an indicator of task switching ability (Milner 1963). All seven frontal lesion patients who showed deficits on one or more executive function tests were impaired in the WCST, except for patient F1, who refused this test after several incorrect sorts. In the posterior group, four patients showed deficits on one or more of these tests, and three of these were impaired on the WCST[10]. In keeping with their apparently lower premorbid IQ, posterior lesion patients had poorer verbal fluency than the frontal group as a whole, but the left frontal patients showed a deficit on this test relative to the right frontal group. However, the difference between the patient groups, in terms of how many individuals showed executive impairments, was not reliable[11].

The examination of dysexecutive behaviour using the DEX questionnaire did not reveal either any reliable group differences or any individuals with poor performances, although there was an overall tendency for patients to score higher on both ratings than controls. This meant that links between switching or other executive impairments and behaviour could not be examined in this study. It was clear that either the patients did not have any behavioural problems of this sort, or that the test was not sensitive enough, or both. The former was certainly possible, since not all patients with brain damage (Wilson, Alderman et al. 1996) or with frontal lobe damage (e.g. Baddeley, Della Sala et al. 1997) show this pattern of behaviour. As to the latter, the DEX was not designed as a clinical test for dysexecutive behaviour, but introduced to assess the validity of the Behavioural Assessment of the Dysexecutive Syndrome battery (BADS) (Wilson, Alderman et al. 1996). As such, it would not be expected to be a sensitive means of classifying individuals as dysexecutive or not. However, it is possible that patients with such deficits were overrepresented in Wilson et al's sample. The aetiologies of their brain damage were various, but the majority (59%) had had closed head injury. Dysexecutive behaviour may be particularly common in such

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[10] Note that P12 deficit on both parts of the Trail Making Test, and P9's on the Stroop were not counted as evidence of executive impairment. In the case of the former, this was because only selective difficulty with part B of the test indicates such problems (Lezak 1995). In the latter case, it was because this patient showed naming difficulties on the Mini Minnesota which could have accounted for her low scores in this version of Stroop test, which measures incongruent trial performance only, i.e. colour naming (Trenerry, Crosson et al. 1989).

[11] $\chi^2(1) = 1.97$, n.s.

patients, regardless of the presence of overt frontal lobe damage (see earlier footnote, and McKinlay and Gray 1992). It may therefore be that the questionnaire is only able to pick up fairly gross behavioural variation even in group comparisons.

The broader implications of these characteristics of the present groups will be considered below, in the light of a discussion of the findings of Experiments 6a and 6b.

Task switching and cognitive control

Task-specific preparation and switching

The effect of preparation on progressive task repetition benefits for RT found in Experiments 4 and 5 was again replicated. However, there was no evidence in frontal lesion patients of the predicted inability to prepare for a specific task. The task switching/ repetition 'cost' at long CTI was, in fact, equivalent in all three participant groups, suggesting that task-specific preparation during the cue-target interval was equally efficient, after correction for overall speed. The residual cost in the posterior group was not out of line with the performance of young adults in other studies in this thesis, particularly as it was not present for univalent stimuli alone.

The absence of deficits means that nothing can be concluded about the control mechanisms involved in preparing for a task switch. Can any useful interpretation of this aspect of the patients' performance be made? It is possible that this group of patients with frontal damage simply were not dysexecutive, and therefore did not show deficits in goal-directed behaviour. Certainly, there was no sign of dysexecutive *behaviour* in the frontal group by the measures used, and the methodological issues raised by this distinction are discussed further below. However, there was evidence in half of the individuals in that group of impairment on one or more tests of executive function. Having examined the performance of those patients who failed the Wisconsin Card Sort Test in detail, it is possible to say that poor performance on this test is not associated with a difficulty in using external behavioural cues per se. This does not, of course, rule out the possibility that goal neglect, manifest only where the cueing method is in some sense 'weaker', may contribute significantly to difficulties on the WCST. The present study therefore supports the case for further investigation of the influence of the type of external cueing on the control of action (Allport 1980; Duncan, Emslie et al. 1996). Interestingly, in Rogers et al's (1998) study of switching in frontal lesion patients, the use of 'weak' task cues (and a long RSI) did not affect the

left frontal group's deficit. The latter deficit was, however, dependent on the presence of task-irrelevant stimulus attributes. This is consistent with evidence cited in this thesis, and elsewhere, that task-preparation and interference have independent effects on the control of switching (Rogers and Monsell 1995; Meiran 1996). Finally, although Alivisatos and Milner (1989) found an impairment in the use of advance information in a spatial task, in a group of frontal lesion patients, the present findings are consistent with the existence of dissociatioins between spatial and non-spatial attentional control.

Cognitive flexibility

The second prediction for the RT task was that patients with frontal lobe damage would be 'stuck' in the switched-from task set, and would therefore have difficulty in benefiting from task repetition. Again, no group differences were found. Patients and controls benefitted to the same extent from task repetition after allowing for baseline speed and accuracy. Frontal lesion patients also did not 'fail to switch' (i.e. make errors on incongruent switch trials) more often than posteriors; in fact there was some trend in the opposite direction, in line with the posterior group's overall poorer performance.

Experiment 6b, the application of a pencil-and-paper test based the one used by Sandson and Albert in their 1984 study, also addressed the issue of being 'stuck' in a previous task set or response tendency. Again, no evidence was found in the patients either of perserveration of the shape copying task rule, or of inappropriate repetition of the most recent response. Interestingly, in this test both repetition and alternation errors appeared to be as frequent in the 'establishing set' part, where participants had to write 'circle' or 'square' under the shapes, as they were in the reversal part of the test. This suggests that the difficulty of the test was affected by the familiarity of the task as well as by the reversal of S-R mappings. However, the frontal lesion group made somewhat *fewer* errors in all parts than posterior patients or controls.

The analysis of previous trial stimulus valence effects in the present study also did not reveal any impairments of control in the frontal lesion group. Rogers et al's (1998) finding of RT slowing where there was interference on both the previous *and* current trials, in patients with (left) frontal lesions, was not replicated. There appeared to be a similar deficit in the posterior group in this study, as they showed RT slowing on *all* task switch trials with previous trial valence. However, this was found to be secondary to their lower NART IQ scores, therefore making it difficult to draw any inferences

about possible underlying processes.

Switching task set or changing responses was therefore no more difficult for the patients with brain damage in this study than it was for the healthy controls. This was the case even for those patients with deficits on the WCST, suggesting that impairments on the latter test were not mediated by a simple difficulty in switching. However, the present data cannot speak to the question of whether these individuals failed the WCST because it was in some sense a more 'difficult' test of switching than the tests used in Experiment 6, or whether their deficits had other causes, such as goal neglect[12].

Effects of strooplike interference

There was no evidence of a difference in the groups' ability to control interference in general (irrespective of task switching). Although the raw RT data appeared to suggest such a deficit in the posterior lesion group, this was found to be explainable by their overall slower responding. There was certainly no evidence of a selective frontal impairment in the control of interference. Although a susceptibility to such interference is often regarded as being typical of patients with frontal damage, and has been localised to that part of the brain by some studies (e.g. Pardo, Pardo et al. 1990), other experiments have not supported such a link (Stuss, Benson et al. 1981; Foster, Eskes et al. 1994). However, because no task switching impairments were found in this study, it is not possible to say to what extent these, where found, may be secondary to such broader executive control deficits.

Broader implications

The present study did not find evidence of selective deficits in processes related to task switching, and the results therefore cannot inform cognitive theories of task switching, as originally intended. Because there was no significant dysexecutive behaviour in the patient participants, it is also not possible to further the understanding of links between executive functioning and behavioural difficulties. Can particular reasons be highlighted for these null findings, and what are the implications for future lesion studies in this area?

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[12] There was no sign that patients with deficits on the WCST produced more perseverations in Experiment

The approach adopted here depended partly on the fact that frontal lobe patients may often have relatively selective deficits in executive control, and partly on claims made that they show impairments specifically in task switching. This does not require that all frontal damage leads to executive dysfunction, or that no other brain areas are involved in executive processing. The aim was simply to obtain sufficient individuals with task switching deficits to examine switching function in detail. In the next chapter further comments will be made about the wider issue of the role of executive control in switching, in the light of evidence from all of the studies in this thesis. But for the present experiment, the important point is that the patients recruited did not show any impairments in task switching on the RT measures used. They also did not display dysexecutive behaviour as measured by the DEX questionnaire. They did, however, show evidence of executive dysfunction on standardised cognitive tests, particularly the Wisconsin Card Sort Test. It therefore appears, at least, that deficits in the ability to switch, *as operationalised in this speeded response test,* are not a major cause of poor WCST performance in patients with frontal lobe damage. It is, of course, possible that this is because the present switching test is insensitive to the relevant deficits, or the patient numbers were insufficient. The latter seems unlikely since Rogers et al's (1998) study compared the performance of twelve patients with frontal lesions of mixed aetiology and location with healthy controls, and detected a selective deficit in the six left frontal patients. That experiment used four blocks of 37 trials, fewer than the present study. The former is difficult to assess, but there is no a priori reason to believe that the present tasks were less sensitive than those employed by Rogers and coworkers.

Could the patients for the present study have been selected on a different basis that would make positive findings (of deficits) more likely[13]? As seen from the review of the literature, selection on purely anatomical criteria can be fruitful, as numerous studies comparing frontal lesion patients to posterior lesion controls have demonstrated (e.g. Milner 1963; Rogers and Monsell 1995; Godefroy, Lhullier et al. 1996). However,

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6b.

[13] This is not intended to be an exhaustive list of possible methodologies. Other, non-anatomical clinical criteria can also be used to study associations and dissociations of control functions, by providing a population with deficits that are to some extent selective. For example, this has been done with patients with early Alzheimer-type dementia, in the area of working memory, since they appear to have particular difficulties on tasks in which the central executive is thought to be involved (e.g. Baddeley, Cocchini et al. 1999).

perhaps it would be better to select from a more circumscribed anatomical group, say (on the basis of Rubenstein and coworkers' studies) those with lesions involving the DLPFC. In a study with more time to recruit patients this could be helpful, provided that care were taken not to confound the hypothesis under consideration with the selection criteria [14]. In other words, such selection could help ensure that there were more patients in the study with impaired switching performance, but such a finding would not of itself implicate the DLPFC *specifically* in task switching. Switching deficits in patients with lesions there could still be secondary to impairments in other processes, just as the difficulties shown by Rogers et al's (1998) left frontal group could have been the result of the presence of interference between tasks, rather than the requirement to switch per se.

Another potentially useful method in such studies is selection on the basis of a specific pattern of cognitive deficit, such as failure on the WCST. This approach has successfully been used by Owen et al, who have based strong claims for a role of the frontal lobes in task switching on the finding that patients with frontal damage who failed the WCST also had difficulty making extradimensional shifts (EDS) in another rule acquisition task (Owen, Roberts et al. 1991; Owen, Roberts et al. 1993, see Introduction to this chapter). However, it is important to remember that deficits in both tests could, in fact, be secondary to broader impairments of cognitive control connected with, for example, the requirement to monitor feedback and deduce novel task rules. As stated above, the present findings lend some support to such a notion. The same caveat also applies as earlier about independence of the selction criteria and the cognitive hypothesis being evaluated.

A third possible way of selecting patients for studies of executive control is on the basis of behavioural criteria. The establishment of links between poor performance on tests and behavioural problems, only a secondary aim of the present study, clearly requires that patients show such problems. This was not the case with the present groups. Baddeley, della Sala et al (1997) divided frontal lesion patients into those with and those without clinically significant dysexecutive behaviour, and found dual task deficits only in the former. This method might be useful for elucidating switching function as well as determining whether it is linked to behaviour, without running the risk of

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[14] See Shallice (Shallice 1988) for discussion of the pitfalls of the 'symptom complex' approach to neuropsychology.

entirely null findings. However, twice the number of patients with selective frontal lobe damage would be required, since comparing frontal lesion patients with and posterior lesion patients without behavioural impairment would make it impossible to tell whether the lesion site, the behaviour, or both, were associated with the cognitive deficits.

Conclusions

This study has examined the task switching performance of a group of patients with frontal and posterior brain damage. Although they did not exhibit significant dysexecutive behaviour, as reflected in questionnaire ratings, half of the frontal group (and a few of the posterior) did show signs of executive dysfunction as indicated by deficits on the Wisconsin Card Sort Test and other standardised measures. Despite this, no impairments of task switching were found on the speeded response test, and no evidence of stuck-in-set perseveration on a pencil-and-paper test. The implications for neuropsychological studies of task control and switching in frontal lobe patients have been discussed, and in the next chapter the broader implications of the findings of all the studies in this thesis for theories of task switching will be considered.

Chapter 6

General Discussion

In this Chapter, the broader implications of the experiments reported in this thesis will be considered. The starting point of this discussion will be a brief summary of the factors that have been found to affect task switching in the present investigation, and the contribution of these findings to knowledge in this area. However, instead of assuming that switch costs are the result of cognitive control operating at the time of a switch, phenomena associated with switch costs will be reviewed specifically from the point of view of questioning this assumption. The status of theories of task switching will be explored, with brief reference to some new evidence from studies published by other investigators since the present investigation was completed. This will lead on to a consideration of more general issues of control raised by the investigations reported here, their implications for the theory and methodology of executive function, and to some conclusions about the future usefulness of studying task switching as a specific locus of cognitive control.

The investigations reported here set out to explore the role of executive control in task switching. Previous work had shown that task switching takes time, but that more than one process is involved. The suggestion that global cognitive control might be required to prepare for a switch, seemed to tie in with concepts of executive function proposed by Shallice and by Baddeley (Norman and Shallice 1980; Baddeley 1986; Shallice 1988; Baddeley 1996). The first three studies, in Chapter 2, used a novel non-spatial switching task, based on Meiran's (1996) precueing paradigm, to look at this. Executive control was operationalised as an involvement of the central executive of working memory, as indicated by selective impairments of dual task performance. However, no evidence was found that the CE was required to prepare for a task switch, although preparation did reduce switch costs, as other investigators had found. Instead, an unexpected effect of CE load suggested that executive control might be involved in maintaining *improved* performance on task repetition trials. This was only found where implicit task cues were used. This result led to the temporary setting aside of questions of executive control, while an investigation of task repetition performance was conducted, in Chapter 3. The finding of progressive effects of task

repetition, that did not stop after the initial switch trial, had a number of implications for theories attempting to explain switch costs. These will be considered again, below.

The next two studies, in Chapter 4, returned to the role of working memory in executive control. They took the findings of the previous chapter as a starting point, and examined the effects of CE load on task repetition as well as task switching, with both explicit and implicit task cues. No evidence was found that the CE exerts inhibitory control during task repetition specifically, as had been suggested earlier. Instead, the disruption of performance under high CE load with implicit cueing in Chapter 2 were thought to be due to a failure of updating of recent-task information in short term memory. Implications of this for executive function in working memory will be reviewed below. The experiments in Chapters 3 and 4 also broadened the scope of the findings of the studies in Chapter 2 by using a different non-spatial switching task, within the same paradigm.

The final experiment reported, in Chapter 5, applied a somewhat different method to the investigation of control in task switching and repetition. A neuropsychological lesion study investigated the performance of a group of patients with frontal and posterior brain damage on the switching and repetition task used in Chapter 3. The frontal lobe group were expected to display deficits of executive control, and dissociations between groups in terms of switching and repetition were predicted. However, despite evidence of executive impairment in this group, and to a somewhat lesser extent in the posterior lesion group, the patients showed no impairments in switching and repeating tasks, compared to healthy controls. The implications of these findings for the understanding of frontal lobe function, and some considerations of neuropsychological method, were discussed.

Processes involved in task switching

The results of studies 4, 5 and 6 have all indicated that 'switch costs' are generated by processes affecting task repetition as well as task switching. There are two issues that require consideration here, one specific and one with more general implications. The first issue is to what extent the effects of preparation and of interference on switch costs are independent of one another, and what this means. The second issue, already alluded to above, is whether task switching can usefully now be regarded as a specific 'locus' of cognitive control.

Components of switch costs

A consistent finding here has been that task-specific preparation is possible, and time to prepare for a trial leads to a greater RT speedup when the task switches than when it repeats. On successive task repetitions, up until at least the fifth, this effect of preparation gradually reduces. The second consistent finding has been that the RT slowing caused by strooplike interference between the two switched-between tasks is less on a task repeat than on a task switch trial, and that this effect, too, continues progressively over the first few task repetitions. In Experiment 4, the effects of these two factors on task repetition were additive, rather than interactive, suggesting different underlying processes. This was consistent with findings in the literature. There were some departures from this pattern in Experiments 5a and 5b. In Chapter 4, these were considered from the point of view of specific factors operating in those studies. It was suggested that the absence of a task repetition effect on no-crosstalk trials in Experiment 5a was probably due to participants responding to these trials, some of the time, as a 'third task'. The exceptional nature of this finding in the literature was highlighted by reference to two studies that have found reliable switch costs in the absence of interference between tasks (Rogers and Monsell 1995; Meiran 1999). In Experiment 5b, a high overall error rate was thought to be responsible for the fact that no effect of task repetition remained on RT at long CTI, although there was such an effect on errors. In Experiment 6a, both patient groups, as well as healthy controls, showed clear task repetition effects that were influenced additively by preparation and interference. It therefore appears that the studies reported here have confirmed the evidence cited in Chapter 1, which suggests that these effects on switch costs are independent (Rogers and Monsell 1995; Meiran 1999; Rubenstein, Meyer et al. In press). **Table 6.1** summarises the findings of the experiments in this thesis, comparing the effects of these two main factors on affect task switching and task repetition in the different studies.

EXPERIMENT & TITLE	EFFECT OF PREPARATION	EFFECT OF CROSSTALK	RESIDUAL 'COST" WITH CROSSTALK?	RESIDUAL 'COST" WITH NO-CROSSTALK?	EXPERIMENT-SPECIFIC FACTORS
Expt. 1 – switching only, explicit cues	Reduced switch cost	Did not increase switch cost	Yes	Yes (on neutral trials)	Probable crosstalk effects with 'neutral' trials, not intended
Expt. 2 – switching, explicit cues & load	Reduced switch cost	Increased switch cost	Yes	No	
Expt. 3 – switching, implicit cues & load	Reduced switch cost	Increased switch cost	Yes	No	
Expt. 4 –repetition, explicit cues	Reduction in task repetition benefit	Greater task repetition benefit	Reliable task rep. effect at long CTI with crosstalk	No task rep. effect at long CTI in absence of crosstalk	
Expt. 5a – repetition, explicit cues & load	Reduction in task repetition benefit	Greater task repetition benefit	Yes	No	
Expt. 5b – repetition, implicit cues & load	Reduction in task repetition benefit	Greater task repetition benefit – but only reliable for errors at long CTI	At long CTI, task repetition effect sig. only for errors.	No	Apparent interaction of preparation/ crosstalk effects prob. due to high error rate
Expt. 6a – repetition, explicit cues, patients	Reduction in task repetition benefit	Greater task repetition benefit	Yes	No	Though n.s. for frontals & controls, TRENDS

Table 6.1. A comparison of task switching and repetition effects in the seven speeded-response experiments reported here. Only RT data are mentioned except where specified. The switch cost refers to effects on the RT difference between switch trials and all task repeat trials (Expts. 1 – 3). The task repetition benefit refers to the overall effects on RT between the switch trial and the 'last' (5^{th} – 8^{th}) task repetition. See text for further details.

Task cueing effects

Further evidence is available to support the contribution of two separate processes to switch costs, and task repetition benefits, from studies of different task cueing methods. Rubenstein et al (in press) demonstrated additivity of task cueing, and operation complexity, on task alternation costs. In this thesis, effective task-preparation has been demonstrated with implicit precues that give the instruction to 'switch' or to 'stay', as well as with explicit precues naming the task. Although effective, this preparation consistently takes longer with implicit task cues, and the effect is independent of the effect of crosstalk on switching and repetition. Task-preparation has not been found to involve the CE with either cueing method. Note that there were other effects of implicit versus explicit cueing that were not affected by cue timing (CTI), but which involved crosstalk and were influenced by central executive load. These effects will be discussed later.

Some possible mechanisms for the influence of cueing on task-preparation have already been considered in detail in Chapters 2 and 4. It was proposed that implicit cues are associated either with slower retrieval of the correct task rule on each trial, with slower task rule activation, or with cumulative partial task rule activation. The process responsible did not depend on verbal working memory representations, or on the central executive. The involvement of an extra processing step is possible, but this is probably a less parsimonious explanation (see Chapter 4).

The only other investigations of the effects of task cueing on *components* of switch costs were those of Rubenstein et al (in press). As in earlier studies such as that of Jersild (1927), a list-method was used and an increase in alternation cost found. In the context of the authors' production system model of switching, this was thought to result from slower goal shifting when cues do not give explicit information about which task is to be performed. Their studies did not examine preparation, but the model also predicts that this affects goal shifting. This is consistent with other data on task switching considered in a general discussion of the EPIC model of adaptive executive control (Kieras, Meyer et al. 1999)). In this review, Kieras et al also suggest that LTM retrieval processes are involved in goal shifting. Rubenstein et al (in press) claimed that rule complexity effects, which affected alternation cost independently, slowed the rate of the second control process, rule activation. The model predicts that irrelevant character effects should influence rule activation, too, but this prediction remains to be

tested. However, the present findings were consistent with their model in that task cueing interacted with task preparation, and both of which are said to depend on goal shifting, while crosstalk effects, said to depend on rule activation, were independent of both.

Regarding the two putative 'components' of switch costs, it is reassuring that the evidence for dissociations obtained from time-course evidence in the present studies, and elsewhere, converges with evidence of dissociations from additive-factors effects. However, these considerations do not address the question of how the task control processes identified here relate to cognitive control beyond task switching. The extent to which these represent 'switching processes' is clearly in some doubt. This central point will be considered next.

Task repetition benefits and theories of switching

It has been implicit here from the outset that 'task switch costs' could equally well be referred to as 'task repetition benefits'. In the literature, both terms have been used, and the usage has tended to reflect the investigators' theoretical inclinations. For example, studies concerned with 'why switching takes time' have examined switch costs (Jersild 1927; Spector and Biederman 1976; Allport, Styles et al. 1994; Rogers and Monsell 1995; Rogers, Sahakian et al. 1998; Meiran 1999; Rubenstein, Meyer et al. In press), whilst those concerned with RT repetition effects more generally, and not primarily with cognitive control, have looked at the benefits of repetition (Shaffer 1965; Shaffer 1966; Shaffer 1967; Biederman 1972; Duncan 1975). Why has this difference in performance been attributed to extra processing on task switch trials? Some direct evidence for this has come from experiments by Monsell and coworkers.

Stimulus-cued completion and task repetition effects

In Chapter 3, Rogers and Monsell's stimulus-cued completion account of task switching was reviewed in some detail. It was claimed, essentially, that a task switch took place in a single trial, but that one of the two processes involved had to wait to begin until the task stimulus arrived. The findings that this two-component model of switch costs was based on need now to be evaluated in a wider context. The first of these was that, when switching was performed between predictable runs of 4 trials, RT did not continue to improve after the first trial in the run, the switch trial (Rogers and Monsell 1995, Expt. 6). This result was replicated more recently for runs of up to 8 trials, and

for different response-stimulus intervals (Monsell, Azuma et al. 1998). However, Experiment 4 demonstrated that a task switch does not always occur 'at once', and the finding of progressive RT benefits of several task repetitions has been replicated several times in this thesis, as well as by investigators using other paradigms (Brown and Marsden 1988; Salthouse, Fristoe et al. 1998; Meiran, Chorev et al. In press).

It was argued in Chapter 3 that there are two possible reasons for the apparently discrepant findings. The first is that, because of the predictability, preparation for an upcoming task occurs to some extent on the *preswitch* trials in the alternating runs procedure, such that the performance benefits from task repetition are offset by this 'advance cost'. This is essentially an empirical question, and although a similar claim has been made by Meiran et al (in press), there is as yet no direct evidence to suggest that this occurs. De Jong's (1995) experiments do provide some indirect support for this possibility, however. It was shown that particpants could prepare for a particular *order* of performance of two tasks, one with visual and one with auditory stimuli, and that there was a cost to switching the order of performance.

There is some evidence that, where participants can see the upcoming stimulus, they can prepare for the next trial whilst responding to the previous trial (see Spector and Biederman 1976).

Without more direct evidence of advance costs in the predictable runs paradigm, however, Monsell and coworkers have a counter-argument. They have suggested that their studies are more informative because the findings are not contaminated with trial-to-trial uncertainty about which task is to be performed (Rogers and Monsell 1995). Perhaps progressive RT benefits of repeating the same task represent a gradual move on the part of participants from expecting a switch at any time to expecting the same task to continue, and consequent faster performance. 'Uncertainty' is, however, far from being a straightforward concept. To the extent that it is a subjective state of individuals, for example "if the task is unpredictable the participants are uncertain", it has little explanatory value, as one could equally well claim that such uncertainty should be constant throughout a block of random task trials, where the probability of a switch and a repeat remains constant. Any more helpful notion of uncertainty here resembles closely the concept of preparatory state, or 'set'. If the progressive repetition effects in the present paradigm are said to result from varying task uncertainty, and thus equivalent to varying degrees of preparation for a particular task, this is hardly in conflict with the account proposed in Chapter 3.

It is also difficult to see how the finding here of preparation-resistant RT benefits of task repetition, that also accrue over several trials, can be explained as variations in 'uncertainty'. This seems self-evident within Rogers and Monsell's strict endogenous versus exogenous framework of control, but it should be noted that what can be prepared may not be so clear-cut in other situations, so this argument may not generalise well to results from other paradigms.

The second possible reason that Monsell et al's studies and the present studies showed different effects of task repetition, over several trials, is that a task switch *can* indeed occur 'at once', but *need* not, and that whether it does this depends on the context. Before considering the implications of this more conservative interpretation of the findings here, the status of the second phenomenon on which Rogers and Monsell's (1995) stimulus-cued completion account of switching is based, the 'residual switch cost', must first be assessed. The studies in this thesis did not set out to focus on this directly, but it has become clear during the course of this thesis that it is not an invariant finding. In Rogers and Monsell's experiments, a residual cost was always found, even on trials with neutral irrelevant crosstalk, although it was greater on trials and in trial blocks where there was crosstalk (Experiments 1, 2 & 4) In the present experiments, the only finding of a residual cost of switching, or a task repetition effect at long CTI, in the absence of any task-irrelevant characters, was in Experiment 3 (see **Table 6.1**). In the presence of crosstalk, there was evidence of residual costs in all of the studies except for Experiment 5b where, as discussed in the previous Chapter, this was probably influenced by a speed-accuracy trade-off.

The relevance of this to the broader argument here is as follows. If completion of a task switch takes place automatically after the arrival of the first new task stimulus, one would expect a difference at long CTI between the switch and the first task repetition, regardless of crosstalk. Although the present studies did not make this pairwise comparison, a reliable reduction in task repetition RT for no-crosstalk trials over several task repetitions would certainly be expected. A switching process completed by the arrival of the stimulus for the *current* task is logically different from a process dependent on the presence of interfering information from the *other* task. On the contrary, such a pattern of a 'residual cost' only with crosstalk, as found here, is more suggestive of some stimulus-cued competition between tasks, even a sort of partial 'switching back' to the to-be-switched from task. The latter could explain the increased switch cost with crosstalk, too. Stimulus-cued completion cannot therefore

account for the present data.

A further possible defence of this account could be made on the basis of Rogers and Monsell's finding of residual switch costs on neutral trials. However, this could be explained by the difference between 'neutral' crosstalk and no crosstalk. It will be recalled that the 'neutral' stimulus attributes used in Experiment 1 were associated with switch costs of similar magnitude to the congruent and incongruent ones. For that study, the criterion of 'neutrality' of Rogers and Monsell had been adopted, that irrelevant characters should not be associated with a *response* in the competing task. However, the findings implied that interference could arise from attributes that were simply associated with the competing task, without any specific response mapping (see Monsell 1996). Rogers and Monsell's residual switch costs on neutral trials could therefore be the result of weak crosstalk effects, rather than stimulus-cued completion. Again, this needs empirical assessment. If this hypothesis is correct, Monsell and coworkers' position reduces to the point that the processing for a change of task *can,* in their paradigm at least, be carried out 'at once'. This, however, is not the same as stimulus-cued completion, since it can also, in different circumstances, occur partially and/ or gradually. It is also not equivalent to the claim that only one mechanism is involved in task switching. As stated earlier, there is plenty of evidence to the contrary.

Task repetition effects and other models of switching

It is not only the stimulus-cues completion theory that encounters difficulties explaining the finding of task repetition effects over several trials. As already explained, Rubenstein et al's (in press) production system model accounts well for the additive and interactive effects of various manipulations on task switching. However, it does not at present allow for the fact that all of these effects, with the possible exception of the difference between implicit and explicit task cueing, can occur in a graded fashion over several task repetitions. There was an assumption that a switch occurs at once in Rubenstein et al's (in press) study (see Chapter 3). Kieras et al state, more explicitly, that one constraint on processing during switching tasks is that "at each moment, symbolic S-R mapping information for performing just one task is kept in WM" (Kieras, Meyer et al. 1999). Perhaps it would be possible to maintain only one task rule in WM, but for this to be set up faster the more times that task had just been performed, as if setup was positively primed. This would be consistent with an explanation of the graded task-preparation effects found here in terms of speed of activation or retrieval

of task rules. The additional repetition-dependent slowing effect at short CTI due to 'switch' and 'stay' cues might affect the same process, or demand a specific rule-retrieval mechanism. All of these are ultimately empirical questions.

Task-set inertia and related mechanisms

It will be recalled that the first of the recent crop of studies of switching and executive control, that of Allport et al (1994), ended by proposing a non-executive account of switch costs, based on task set inertia (TSI). Stimulus-cued completion was originally conceived as alternative to TSI, designed to accommodate the findings of preparation for a switch accompanied by residual costs. More recently Allport and Wylie, have revised the TSI account, since the carry-over effects responsible for the findings of Allport et al (1994) have turned out to be due to several underlying mechanisms, not all of which operate at the level of the whole 'task' (Allport and Wylie 1999; Allport and Wylie In press). These findings can perhaps point the way towards explaining the part of switch costs, and of task repetition benefits (depending on the paradigm), that is affected by the presence of irrelevant characters. Viewed another way, the consistent effect of crosstalk on task switching and repetition in the present studies (see **Table 6.1**) means that the more the same task repeated, the smaller the interfering effects of crosstalk on RT became. The presence of crosstalk on a previous trial was also associated with improved performance in the face of interference on the next trial, although the relationship between this and the former effect was not clear (see Chapters 3 and 4). In Allport et al's original (1994) study, the recent performance of other possible tasks that used the same stimuli caused slower switching between current tasks. As explained in more detail in Chapter 3, Allport and Wylie have since demonstated both positive and negative priming effects at the level of the task set or rule, which contribute to the fact that task 'reconfiguration' (a switch) is not complete in a single trial (Rogers and Monsell 1995; Allport and Wylie 1999; Allport and Wylie In press). These findings mean that the increase in switch costs on trials with task-irrelevant crosstalk probably occur because of both prior inhibition of aspects of the currently relevant task set, and current activation of aspects of currently irrelevant ones.

It therefore appears that both the 'preparatory' (or cueing-influenced) and the crosstalk-influenced components of switch costs in fact derive from processes that operate, or at least can operate, on task repeat trials as well as task switch trials. The absence of interactions with each other and with central executive load suggests that

these processes are, essentially, local control mechanisms. This is particularly consistent with the theoretical accounts of Rubenstein et al and Allport and Wylie (Allport and Wylie 1999; Allport and Wylie In press; Rubenstein, Meyer et al. In press). What the processes have in common with one another is that they operate at the level of the task set or rule. Although no attempt has been made here to address the issue of "what is a task", the consistency of findings in the literature, across experiments where participants switch between a range of very different tasks, indicates that there is a sense in which this does not matter in understanding switch costs. This fits with the usual assumptions made by concepts of task control, but is as consistent with the suggested pattern of local control as it would be with more global processes. Clearly, consistent patterns across domains can result from dedicated local processes as well as from a unitary mechanism. For example, the phenomena of negative priming, even inasmuch as they appear to result from the same *kind* of process, are not thought to be carried out by a single 'negative priming system' (Houghton and Tipper 1996).

Theories & study of executive control

Have the studies in this thesis provided any useful insights about the nature of executive control, either in relation to task switching or more generally? It is not only the 'positive' findings, but also important null findings, that can be helpful in addressing such general issues.

Task preparation and control

One of the starting points for this series of investigations was the idea that the ability to prepare the cognitive system to perform a particular task is an important component of goal-directed activity. In the context of task switching, this had been proposed by Allport et al (1994), and evidence later provided by Rogers and Monsell (1995) and Meiran (1996). Rogers and Monsell suggested that preparing for a switch was under 'endogenous' or executive control, since it appeared to be strategic. Meiran (1996) emphasised the importance of proactive processing following an explicit task cue, but did not demonstrate strategic effects. Fagot, cited by Rubenstein et al (in press) has made a distinction between 'goal-setting', controlling *which* task is performed, and 'performance-readiness', which is reflected in the speed of performance (Fagot 1994). The former is assumed to involve top-down control, whereas the latter can result from the processing of prior tasks (Allport and Wylie In press). In the literature on attention

and control, preparation for a particular task has also often been assumed to be executive in nature, in the form of behavioural 'planning' (e.g. Shallice 1982) or 'programming' (Luria 1973; Shallice 1988).

The studies reported here have, as described above, suggested that advance task processing is not executive, and does not occur exclusively when the task being prepared for is different from the one most recently performed. The suggestion that this might not rely on some central mechanism or resource is in fact consistent, as pointed out in Chapter 1, with ample evidence that preparation for performance using advance information can take place at different stages and levels of processing. In the switching literature, the aspects of a task prepared in different paradigms have also varied widely, and include relevant spatial (Meiran 1996) or non-spatial stimulus dimensions (Rogers and Monsell 1995), particular task rules (Shaffer 1965; Sudevan and Taylor 1987), or task orders (Dejong 1995). The present findings also serve to emphasise the fact that the effects of different types of task cueing do not necessarily depend on the same underlying mechanisms as does task-preparation.

Another reason why this aspect of the present findings is perhaps not unsurprising is the emphasis, in theories of executive control, on the involvement of central processes in behaviour is the *absence* of direct environmental prompts (Norman and Shallice 1980; Duncan 1986). The explicit task cues here were strong external triggers to a specific task, so preparation using these, as perhaps with other varieties of partial advance information, does not require 'endogenous' control. This, however, is an important dissociation to have demonstrated in practice. Although preparation to process a particular type of stimulus, or perform a particular task, must play an important part in the control of goal directed behaviour, it seems likely that it is distinct from other aspects of such control, and is not executive in nature. Meiran's (1996) task cues always specified the task by name, whereas those used by Rogers and Monsell (1995) (a combination of predictability of task sequence, and spatial cues) did not, and this could account for the apparent strategic nature of task preparation in the latter study. This was why an implicit method of task cueing was also used in this thesis.

Task cueing, working memory and executive control

The effects of different cueing methods in the experiments reported here have some important implications for task control. It was originally expected that 'endogenous'

cueing would slow the 'endogenous' control of task switching, and be associated with an effect of CE load on the latter, if all of these were dependent on an executive working memory process (see Chapter 2). However, the finding that the manipulation of cueing affected task-preparation and CE load effects independently, led to the subsequent abandonment of the former use of the term 'endogenous', which then seemed unhelpfully broad. The effects of cue type on the *use of the cue for task-preparation* have been considered already. This may involve the retrieval of task rules from long-term memory, but does not depend on working memory control processes, as conceptualised in the model of Baddeley (1986) and others (e.g. Jonides, Schumacher et al. 1997; O'Reilly, Braver et al. 1999). What can be said about the effects of implicit task cueing that *did* interact with CE load in this context?

Active maintenance and working memory

The way in which task cueing influenced CE load effects in Experiments 5a and 5b was not consistent with a failure of inhibitory control at the level of the task rule by the CE, originally suggested as an explanation of the findings of Experiments 2 and 3. It was suggested in Chapter 4 that this interaction of implicit cueing and high CE load, which was independent of task-preparation, was the result of a failure of the updating of information in working memory. It was seen that this fits with views expressed in the functional imaging literature about the role of control processes, localised in the prefrontal cortex (PFC), in working memory tasks. It seems that anatomical dissociations of working memory function may converge with the present, and other, functional dissociations. In terms of the modular working memory model of Baddeley (1986), this implies the existence of an updating or rehearsal mechanism that is independent of the phonological loop. The low CE load task, which involved (at least) articulatory suppression, did not disrupt performance in this way. Such a view is consistent with that of Baddeley et al (1999). O'Reilly et al (1999) propose a control function in working memory that is also consistent with the present findings. Control is thought to operate when stored information has to be updated or manipulated concurrently. 'Active maintenance' and updating are seen as important elements of control, particularly when a representation must be maintained (as here and in the n-back task) in the face of subsequent processing of similar kinds of information. This is consistent with other work (e.g. Postle, Berger et al. 1999). Jonides et al further suggest that the n-back task demands control because of a need for temporal coding with inhibition of earlier representations (Jonides, Schumacher et al. 1997).

There are interesting parallels here with studies of 'working memory span' tasks, in which concurrent information processing and storage are required (Daneman & Carpenter 1980). Kyllonen and Christal have found that such task requirements correlate strongly with measures of fluid intelligence, or 'g' (Kyllonen and Christal 1990). Returning to the anatomical association, it has been suggested that this quantity reflects the functioning of a network of processes in the prefrontal cortex (Duncan, Burgess et al. 1995; Duncan 1996). Clearly the present findings do not imply any particular anatomical correlates for the effects of implicit versus explicit task cueing, although it would be interesting in this context to extend Experiment 6a by conducting a parallel study using 'switch' and 'stay' cues in patients with frontal lobe damage. However, these links to broader questions do support the idea of a working memory control process or processes. Whether such processes qualify as domain-independent control depends on a number of factors that require further investigation. One is evidence that they apply across, for example, the visual and the verbal domains. Findings of functional imaging studies in which spatial working memory also activates the DLPFC is consistent with this (Awh, Jonides et al. 1996). However, to demonstrate executive involvement it is also important to show that this convergence is due to a *shared* process, not just different control processes that are both located in the same anatomical area. For this, functional as well as anatomical dissociations are required (Goldman-Rakic 1998). It was assumed in Chapter 4 that the reason the high CE load task had its effect on the use of implicit task cues was because the same control process was required for maintaining and updating information in the serial 7s task as in the switching task.

This discussion has moved away from the question of task switching with good reason. Although the original hypothesis, that preparation for a switch would rely on the CE, has received no support, the task switching paradigm has been a useful one in raising these more general issues. Another putative role for executive control mentioned a number of times in this thesis has been the inhibition of currently irrelevant task representations, or the suppression of inappropriate response tendencies. The concept of such executive inhibition has frequently been put forward in the literature (e.g. Norman and Shallice 1980; Logan 1985; Duncan 1986; Roberts, Hager et al. 1994; Jonides, Schumacher et al. 1997). However, no good evidence has been found that such processes play a role in task switching and repetition in the present paradigm. The proposed link between executive inhibition and cueing effects on task repetition trials in Experiment 3 has already been tested and rejected in Chapter 4. The more

general finding in Experiment 3, of greater overall slowing of RT with task-irrelevant characters than in Experiment 2 was also consistent with such an explanation. However, this effect of implicit task cueing was not replicated in Experiments 5a and 5b. There, differing influences on performance of crosstalk depending on cue type appeared in the error rates in the high load condition. These findings have already been considered and a more specific explanation suggested.

Converging evidence from list method studies

To what extent are the effects of working memory load on task switching here in agreement with findings in studies that have examined this by comparing pure with alternating-task lists? The experiments of Allport and Styles (1990), which did not address the role of the CE directly, have been described in some detail in Chapter 1. However, a series of studies have recently been carried out that specifically asked this question (Baddeley, Chincotta et al. Submitted). Although the results indicated no influence of CE load on switching, certain other secondary tasks, that on the face of it were easier, and initially assumed only to cause articulatory suppression, *did* influence switching.

The CE load tasks used by Baddeley et al (submitted) were random generation and verbal trails. In neither case was the proportional cost of task alternation increased, since there was equal interference with blocked and alternating mental arithmetic. However, in an endogenous cueing condition, the repetition of sequences of days of the week, or months of the year, did increase the alternation cost. There was some evidence that the effect resulted from a requirement for concurrent retrieval from LTM in these tasks, and it was suggested that this was also important in switching in the absence of explicit task cues. Given the findings of the present studies, it seems likely that this effect was mediated by a slowing of task-preparation by concurrent LTM retrieval. As the list-method was used, and preparation for a switch not measured, such an effect would have been reflected in the overall task alternation cost, as was found (Baddeley, Chincotta et al. Submitted). One caveat here is that in the present study both low and high CE load tasks would have required some concurrent LTM retrieval, but the former did not affect task-preparation. Possibly this discrepancy is a matter of degree, related to the difficulty of, or time taken for, retrieval of facts from LTM in the arithmetic tasks of Baddeley et al (adding or subtracting digits) compared with the low CE load task of the present studies (adding only 1 with each step), and the very small effect on switch cost of the repetition of the word 'the'.

It is less straightforward to determine how the suggested WM updating deficit under high CE load in the present study would have been reflected in the list-method experiments, but on the face of it one would expect an impairment in alternating list performance. It may turn out that this discrepancy with the present findings reflects the different types of 'endogenous' or 'implicit' task cuing, as well as the different task contexts in which switching takes place. However, the data seem at least consistent with two effects of implicit task cueing, a LTM retrieval effect and an effect on the control of WM, both of which can influence performance *while* switching tasks, but are independent.

Varieties of task cueing

An overview of the processes involved in different methods of cueing task shifts is difficult to form, because not enough is yet known. This has become clear in the present thesis from the ambiguities surrounding the notions of 'endogenous' control and 'endogenous' cueing. At the beginning of this thesis, a simple hypothesis was suggested, that nonspatial attention shifting might be separable functionally according to the involvement of endogenous and exogenous control, a distinction generally accepted in the spatial attention literature (Posner 1978; Jonides 1980). A familiar point about executive control is again pertinent here. It is possible that *all* (or some) varieties of attention shift can be controlled endogenously or exogenously, but that all such endogenous control is not carried out by a single process. Evidence of a central executive working memory involvement in endogenously cued preparation for a switch here would have supported the single process interpretation, because of Jonides' (1980) earlier findings of an effect on spatial attention shifts. The findings here were clearly contrary to this.

It is, of course, possible that such control *is* involved in endogenous non-spatial attention switching, but that the 'switch' and 'stay' cues used here did not tap it. Indirect support could be derived for such an idea from a PET study comparing movements that were 'internally' or 'externally' generated (Jahanshahi, Jenkins et al. 1995). The 'internally generated actions' activated the DLPFC, but in this case 'internal' meant that participants generated an action when they chose, rather than at times determined by auditory tones. In other words, an essential quality of 'endogenicity' could relate to choosing *when* to act, rather than selecting the action to-

be-performed. However, this would need to apply also to, for example, spatial attention shifts, to avoid the obvious criticism that the results could have reflected the operation of a specialised 'time-based shifting' mechanism (Coull, Frith et al. 2000). Functional as well as anatomical interactions would also be required, for reasons explained above.

It is quite clear from these considerations that the processes potentially involved in cueing different types of shifts of task are numerous, and the issues raised lie at the heart of several of the important debates in the literature on executive control. One lesson of the present studies has been that it would be as unwise to assume that task cueing processes are on a continuum, such as from 'strong' to 'weak', as it is to assume that they are dichotomous, for example 'exogenous' and 'endogenous'. The processing involved in task cueing is unlikely to depend only on the physical nature of the cues, but also to be influenced by other aspects of the tasks, as by the predictability of switching in the experiments of Rogers and Monsell (1995). Although these issues are complex, the effects of task cueing manipulations are likely to be important in evaluating models of executive control. The implications of the findings of these studies for such models will be addressed next.

Task switching and models of cognitive control

The foregoing discussion has shown how it has been helpful, in this thesis, to distinguish different types of processing underlying 'endogenous control', and goal-directed behaviour. As seen in the introduction to this thesis, the modelling of cognitive control is still at a relatively early stage of development, and some of the implications of the present data have already been discussed. The consequences for some specific models will now be considered, and whether there are any lessons to be learned about appropriate ways to investigate and to model cognitive control. No attempt will be made to give an exhaustive review of this area. A fundamental question needs also to be borne in mind. In the light of these investgations, what can be said about the use, if any, of the notion of a central executive mechanism in understanding cognition?

Norman and Shallice's SAS

The two main models that the present work was based on, following numerous other studies of task control switching, were those of Norman and Shallice (1980), and

Baddeley and Hitch (Baddeley and Hitch 1974; Baddeley 1986). The notion of endogenous control is fundamental to Norman and Shallice's attention-to-action model, although the authors did not use this term. The supervisory attentional system allows the control of behaviour where contention scheduling does not result in the selection of a unique action schema, or results in the selction of one that is inappropriate in current circumstances (Norman and Shallice 1980). The results of the studies here that have used explicit task cues could be explained on this model in terms of the triggering of task schemas by cues and by the imperative stimuli, without reference to the SAS. The implicit cues are more complex, but again, a 'task rule retrieval' schema could be added to the task-specific schemas to explain the extra processing time preparing for a task in this situation. The fact that task-preparation occurs when the task repeats as well as when it switches is, in fact, consistent with such an account. However, some variation in schema activation time would be required to explain the response speed effects of task repetition. The interaction between working memory load and performance overall with implicit task cueing cannot be explained by Norman and Shallice's model, without some additional temporary memory representations. This was the reason that Baddeley and Hitch's (1974) model was also referred to in the design of these studies.

Since schemata are selected either because of the presence of associated environmental triggers, or by the SAS, the endogenous/ exogenous distinction applies quite naturally in the Norman and Shallice (1980) model. However, the present results suggest that endogenous control is not unitary, because of the findings of more than one effect of using an 'endogenous' cueing method. It is worth recalling in this context that the 'switch' and 'stay' cueing method used here was employed by Brown and Marsden (1988; 1991) in their studies of patients with Parkinsons' disease, and let to the demonstration of interesting dissociations in performance. Can the attention-to-action model accommodate these varied findings? Without further specification there is a risk that endogenous control might come to mean whatever the SAS does. In later work developing this model, Shallice has repeatedly stated that the SAS is not unitary, and provided evidence to support some putative subdivisions of its functions (e.g. Shallice and Burgess 1996). However, it remains possible that, for example, 'endogenous' shifts of different varieties of attention are controlled by separate supervisory subsystems. In such a situation, there are two principal difficulties. The first is whether the concept of an SAS still has any theoretical coherence, for example, do the putative subsystems have the same structural relationship to the 'lower'

elements of the model, in particular, contention scheduling? If the answer to this question turns out to be 'no', the term would come to refer to nothing more than all control processes that are not part of individual schemas or of contention scheduling.

The second question is whether, if the concept of an SAS is coherent, it can generate any really testable predictions. As the discussions here show, the study of executive control is reaching the point where the null hypothesis of a unitary control process is becoming something of a 'straw man'. However, to the extent that the notions of endogenous control (see above) and the inhibition of inappropriately activated schemas by the SAS have led to interesting theoretical investigations, the construct can be said to have been useful (e.g. Roberts, Hager et al. 1994). Such a broad construct is unlikely to be falsifiable by a single experiment or series of experiments, because of the nature of its proposed functions, but as with other broad constructs, it may well eventually become too vague to be helpful.

Working memory and the central executive

Turning to the working memory model of Baddeley and Hitch, the results of the present studies have provided some insights into the relationship between cognitive control and temporary memory representations. Most importantly, WM, in the verbal domain at least, has been shown not to be involved specifically in switching between known tasks. This has suggested that the relationship of WM to spatial and non-spatial attention shifts may not be the same (Jonides 1980). However, the effect of the 'implicit' task cueing manipulation on performance has lent support to views of working memory as an active maintenance system, in which control processes are important in roles such as the updating of ordered information (Jonides, Schumacher et al. 1997; O'Reilly, Braver et al. 1999).

The wider implications are not quite the same for the central executive of WM as for the SAS, despite Baddeley's (1986) suggestion that the latter be adopted as a provisional description of the functions of the former (see Chapter 1). Due to its situation in the model and relationship with the WM 'slave systems', subsequent work has in this area has followed a slightly different course in terms of defining possible roles for the CE (Baddeley 1996; Baddeley and Della Sala 1996). However, the main difference in its theoretical status has been that, at least in principle, its role has been *explicitly* to occupy the place of a homunculus in the model, a homunculus that should, over time and with experimentation, become progressively more redundant (Baddeley

1996). As dissociable control processes are defined, the problem of theoretical coherence described for the SAS is avoided if these processes have their own status in a model that is not, unlike that of Norman and Shallice, committed to the notion of central control. There is a risk, however, of the CE construct becoming redundant for a different reason. To be helpful as anything other than a reminder of what is not yet known, it must help generate hypotheses about control that cannot be derived from the functions of the slave systems. For example it is clear that some account is needed of the relationship of short-term and long-term memory, regardless of what role a CE may play in this (see Miyake and Shah 1999).

'Decentralised' models of cognitive control

If the concept of a central cognitive control process is abandoned, are the alternative approaches to theorising mental control more satisfactory? Some investigators have chosen to focus on particular aspects of control, and have avoided attempting to provide cognitive 'theories of everything'. This has clear advantages in terms of avoiding the pitfalls of explaining control as the result of the action of an all-powerful central system. An example of this approach has been the description of different executive processes concerning 'strategies' by Logan (1985). This covers the effects on processing speed and the demands for various 'resources' of four functions, including 'strategy construction' and 'strategy disengagement'. In comparison with the attention-to-action model of Norman and Shallice, an 'incomplete' model can do well in terms of theoretical coherence, and may more easily make detailed predictions, but has a smaller scope (Logan 1985). For example, as Logan points out, his framework does not provide an account of *how*, for example, strategy construction takes place. Models such as this ignore rather than avoid the homunculus problem, but this is in an important sense preferable to confusing the operation of a homunculus with real explanations of behaviour.

More recently, investigators have increasingly aimed to provide theories and models that have a broad scope of explanation, but that do not include all-powerful central processors. One such has already received frequent mention in this thesis. This is the production-system *Executive-Process Interactive Control* or EPIC model of Meyer and Kieras and coworkers (Meyer, Kieras et al. 1995). EPIC takes contemporary computer operating systems as inspiration, and attempts, for example, to resolve conflicts, schedule task and allocate resources without recourse to a unitary controller (Kieras, Meyer et al. 1999). It has a central processor that 'runs' the production rules, and

incorporates a 'declarative' working memory, in which currently active goals and markers of current status are placed. There is no a priori limit to the number of production rules that the system can process simultaneously. The executive processes referred to so far here are themselves production rules that insert or delete items from WM. The model has attempted progressively to account for more complex skills and skill-acquisition procedures. Local control processes ('customised executives') are distinguished from global control processes ('general executives'). The latter, like lower level processes, are entirely algorithmic (Kieras, Meyer et al. 1999). Clearly there is a strong sense in which such systems, however complex and successful, may never completely solve the homunculus problem. Even if well-specified global control is successfully implemented, there is still potentially an infinite regress of control systems, back to the programmer. However, at this level, it may not matter, because, as pointed out by Logan (1985), among others, one may achieve an excellent account of behaviour without doing so. Philosophical debates aside, an approach such as that implemented in EPIC seems to be desirable because it can account potentially for different types and levels of cognitive control in a coherent way.

There is a further important point to be made here in favour of broad models of cognitive control. As well as there being a danger of being over-general in the specification of control processes, there are potential dangers also of over-fractionation. Duncan and coworkers have made the point that executive control may operate in an important sense as a system, as well as consisting of processes that are dissociable under certain circumstances (Duncan 1996; Burgess 1999). This tendency is shown by the intercorrelations between different control functions that are reflected in the phenomenon of fluid intelligence or 'g', and the fact that this quantity is influenced by frontal lobe damage, and associated with dysexecutive performance in healthy people. It is possible that control processes may not be strongly modular in Fodor's sense, an idea that has also been proposed by Moscovitch in the context of the control of memory (Fodor 1983; Moscovitch 1992). Burgess (1998) has warned of the dangers of paying attention to functional dissociations without heeding functional associations in this context (see Chapter 5).

On this basis, it may be concluded that theorising cognitive control is likely to remain a balancing act between allowing for plurality of mechanisms without sacrificing theoretical coherence. It is therefore likely to be largely a matter of taste, and experimental context, whether one opts, like Attneave (1960) and Baddeley (1996), for

an explicitly homuncular central executive which can be pared down, or, like Kieras et al (1999), for a control structure that avoids this issue directly, but may be at risk of allowing the homunculus to enter by the back door.

General conclusions

This thesis set out to investigate task switching as a particular 'locus' of executive control. The evidence that this was not the case, in the experiments presented here, has suggested a number of important inferences about the ways in which cognitive control operates. Models of task switching have to some extent been able to account for the data, but a more fundamental point has been that these are of limited use unless they can be related to models of control on a wider scale. The present investigations have also touched on, and suggested ways to examine further, certain putative global processes of control.

Understanding control is important if cognitive psychology is to move from an understanding of simple laboratory-based tasks to more complex ones that can better predict performance in the real world (e.g. Shallice and Burgess 1991). But it is clear that not all such points of assumed control require global, executive processes. Mere description of task components is also not enough. As Rabbitt has recently pointed out, the processing that a task *appears* to depend on may be very different from the mode of operation of the actual underlying process or processes (Rabbitt 1999). In the present context, reference to modelling within the EPIC production system framework has been useful in maintaining this distinction (Meyer, Kieras et al. 1995; Kieras, Meyer et al. 1999).

The investigations reported here have shown that the use of specific paradigms such as task switching, to study executive control, need not be self-fulfilling. Theoretical progress can be made in the area of executive control by specifying local control mechanisms as well as by demonstrating the operation of global processes. In this way, although the processing homunculus is not entirely banished, knowledge about the control of behaviour can move forward in a coherent manner.

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Appendices

Appendix A: Instructions given to participants

This is a copy of the instructions for Experiment 2, as a sample. Sheets for the other studies were very similar, depending on the subtasks, the cues, and the requirement, or otherwise, for the performance of concurrent tasks.

INSTRUCTIONS

In this experiment, I am looking at people's ability a/ to switch between doing two things, and b/ to combine doing two things.

Both of these tasks require you to press buttons on the box provided, when certain pictures appear on the computer screen. **The two tasks are:**

1. **Classifying shapes** as triangles or circles
2. **Classifying lines** as vertical or horizontal

The same 2 buttons are used for both tasks (see separate sheet).

In the experiment each picture, appearing **in the middle of the computer screen,** will contain one shape and/or one line. Just before the picture appears, you will be warned whether to pay attention to shapes or lines by **cue words** which appear to the right and left of the centre.

If the words say 'SHAPE', respond to the shape on the picture, and ignore any lines.

If the words say 'LINE', respond to the line on the picture, and ignore any shapes.

Some of the time you will also be asked to carry out simple sums in your head whilst doing the computer task. This will be explained in more detail by the experimenter. You will have time to practice the computer task and the two tasks together before starting.

The computer will let you know if you have pressed the wrong button: Do not try to correct yourself, but wait for it to show the next picture.

PLEASE RESPOND TO THE PICTURES AS QUICKLY AS YOU CAN - BUT IT IS IMPORTANT AT THE SAME TIME THAT YOU KEEP MISTAKES TO A MINIMUM.

Appendix B: Stimuli used in Experiments 4 – 5b

Words

The table below contains the full list of word stimuli used in these studies. Norms are taken from are from Battig and Montague (1969).

	Name	Category rank	Length	Frequency
TOOLS	Axe	28	3	1.85
	Chisel	7	6	1.70
	Hammer	1	6	2.25
	Knife	30	5	4.30
	Pencil	14.5	6	4.05
	Pliers	10	6	1.90
	Ruler	8	5	3.10
	Saw	2	3	2.05
	Mean:	**11.7**	**5**	**2.65**
BODY PARTS	Ear	8	3	3.20
	Finger	7	6	3.35
	Foot	5	4	3.50
	Hand	9	4	3.95
	Heart	15	5	3.00
	Lips	33.5	4	2.85
	Nose	6	4	3.6
	Thumb	45	5	3.1
	Mean:	**16.1**	**4.4**	**3.32**

Picture stimuli

The table below contains the full list of word stimuli used in these studies. Picture stimuli were taken from Snodgrass and Vanderwart (1980), as were picture norms, except where indicated. Category ranks are from Battig and Montague (1969).

	Name	Category rank	Visual complexity	Familiarity
ANIMALS	Camel	28	3.75	1.73*
	Cat	2	3.25	4.00*
	Dog	1	3.38	4.05
	Frog	75	3.42	2.38
	Horse	3	3.82	2.82
	Monkey	32.5	3.90	2.09
	Rabbit	19.5	3.28	2.81
	Sheep	13	3.80	2.86
	Mean:	**21.8**	**3.58**	**2.84**
VEGETABLES	Artichoke	32	3.72	2.29
	Asparagus	9	3.32	2.68
	Mushroom	46	3.12	3.20
	Onion	18	2.85	3.95
	Pepper	26	2.48	3.59
	Potato	5	2.20	3.91
	Pumpkin	46	2.60	1.77
	Tomato	6	1.98	3.64
	Mean:	**23.5**	**2.78**	**3.13**

Indicates value from norms of Morrison et al (1997).

Appendix C: Dysexecutive behaviour questionnaire

Self-rating

TVTC
Thames Valley Test Company

BADS

Dex Questionnaire
Self-rating

Subject's name

Date

This questionnaire looks at some of the difficulties that people sometimes experience. We would like you to read the following statements, and rate them on a five-point scale according to your own experience:

1 I have problems understanding what other people mean unless they keep things simple and straightforward

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

2 I act without thinking, doing the first thing that comes to mind

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

3 I sometimes talk about events or details that never actually happened, but I believe did happen

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

4 I have difficulty thinking ahead or planning for the future

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

5 I sometimes get over-excited about things and can be a bit 'over the top' at these times

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

6 I get events mixed up with each other, and get confused about the correct order of events

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

7 I have difficulty realizing the extent of my problems and am unrealistic about the future

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

8 I am lethargic, or unenthusiastic about things

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

9 I do or say embarrassing things when in the company of others

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

10 I really want to do something one minute, but couldn't care less about it the next

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

11 I have difficulty showing emotion

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

12 I lose my temper at the slightest thing

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

13 I am unconcerned about how I should behave in certain situations

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

14 I find it hard to stop repeating saying or doing things once they've started

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

15 I tend to be very restless, and 'can't sit still' for any length of time

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

16 I find it difficult to stop myself from doing something even if I know I shouldn't

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

17 I will say one thing, but will do something different

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

18 I find it difficult to keep my mind on something, and am easily distracted

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

19 I have trouble making decisions, or deciding what I want to do

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

20 I am unaware of, or unconcerned about, how others feel about my behaviour

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

Independent rating

TVTC
Thames Valley Test Company

BADS

Dex Questionnaire
Independent rater

Subject's name

Date of rating

Rater's name

Relationship to subject

This questionnaire looks at some of the difficulties that people sometimes experience. We would like you to read the following statements, and rate them on a five-point scale according to your experience of _____ [the subject]:

1 Has problems understanding what other people mean unless they keep things simple and straightforward

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

2 Acts without thinking, doing the first thing that comes to mind

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

3 Sometimes talks about events or details that never actually happened, but s/he believes did happen

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

4 Has difficulty thinking ahead or planning for the future

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

5 Sometimes gets over-excited about things and can be a bit 'over the top' at these times

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

6 Gets events mixed up with each other, and gets confused about the correct order of events

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

7 Has difficulty realizing the extent of his/her problems and is unrealistic about the future

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

8 Seems lethargic, or unenthusiastic about things

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

9 Does or says embarrassing things when in the company of others

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

10 Really wants to do something one minute, but couldn't care less about it the next

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

11 Has difficulty showing emotion

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

12 Loses his/her temper at the slightest thing

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

13 Seems unconcerned about how s/he should behave in certain situations

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

14 Finds it hard to stop repeating saying or doing things once started

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

15 Tends to be very restless, and 'can't sit still' for any length of time

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

16 Finds it difficult to stop doing something even if s/he knows s/he shouldn't

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

17 Will say one thing, but will do something different

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

18 Finds it difficult to keep his/her mind on something, and is easily distracted

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

19 Has trouble making decisions, or deciding what s/he wants to do

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

20 Is unaware of, or unconcerned about, how others feel about his/her behaviour

☐0 ☐1 ☐2 ☐3 ☐4

Never Occasionally Sometimes Fairly often Very often

Thames Valley Test Company, 7–9 The Green, Flempton, Bury St Edmunds, Suffolk, IP28 6EL, England.

Appendix D: Test procedure for patient study

The Folstein Mini Mental State examination.

The Mini Minnesota Aphasia Screening test [Powell, 1980 #955].

The Star Cancellation test.

The Hospital Anxiety and Depression Scale (HADS).

The National Adult Reading Test (NART).

Digit span.

The Trail Making Test.

The Stroop Neuropsychological Screening Test.

The Controlled Oral Word Association 'FAS'.

The Wisconsin Card Sorting Test.

The Dysexecutive Behaviour Questionnaire (DEX).

Speeded response switching test (Experiment 6a), with (interspersed in breaks between trial blocks):

Perseveration test (Experiment 6b).